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"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster
vilior quia ex alienis libamus ut apes." Just. Lips. *Polit.* lib. i. cap. 1. Not.
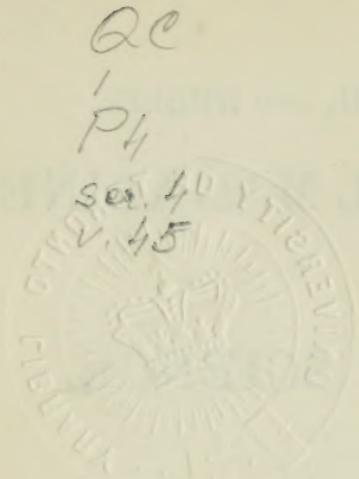
"Meditationis est perscrutari occulta; contemplationis est admirari perspicua . . . . . Admiratio generat quæstionem, quæstio investigationem, investigatio inventionem."—*Hugo de S. Victore.*

—" Cur spirent venti, cur terra dehiscat,
Cur mare turgescat, pelago cur tantus amaror,
Cur caput obscura Phœbus ferrugine condat,
Quid toties diros cogat flagrare cometas;
Quid pariat nubes, veniant cur fulmina cœlo,
Quo micet igne Iris, superos quis conciat orbes
Tam vario motu."

J. B. Pinelli ad Mazonium.

CONTENTS OF VOL. XLV.

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PLATES.

ERRATA.

Vol. 44, page 522, line 5 from bottom, *for* therefore *read* therefor.
Vol. 45, page 160, *for* paragraph 3 of Mr. Hudson's paper (commencing
  " This point " &c.) *substitute* :—

This point can be easily tested experimentally as regards *sound*. Thus
a tense string with amplitude of vibration $= 2$ ought to become inaudible at
twice the distance at which it ceases to be heard with amplitude $= 1$, if the
square of the amplitude be the correct assumption. The relative distances
at which the sounds should be inaudible ought to be as 1 to 0·70715, if the
simple amplitude (not its *square*) be correct.

1. *On Manometric Flames. By* Dr. Rudolph König (*of Paris*)*.
[With Two Plates.]

IN the beginning of 1862 I invented a new method of ob-
servation, which had for its object to make apparent the
sounding air-waves, or, what is the same thing, the changing
density of the atmosphere while penetrated by sounding vibra-
tions, or while itself in a state of vibration, in the same way as
acoustic experiments were able to show clearly the vibration of
bodies which produce the vibration of the atmosphere.

The first apparatus founded on this method was shown in the
London Exhibition of 1862; and since that period I have
invented a whole series of apparatus on the same principles: a
short description of some has appeared in Poggendorff's *Annalen*
for 1864; and others are briefly sketched in my Catalogue of
1865.

The following pages are designed to explain all these appa-
ratus, as well those which have been added since the publica-
tion of my Catalogue, as also the experiments in connexion with
them.

The small instrument, on the use of which my method is
founded, and to which I have given the name of *Manometric
Capsule*, consists of a cavity in a wooden plate, whose orifice is
closed by a thin membrane. Illuminating gas may be intro-
duced into this cavity through a pipe—a second pipe, termina-
ting in a gas-burner, giving means for exit and ignition.

Now, if the air before the membrane be rendered suddenly of

* Translation, communicated by the Author, from Poggendorff's
Annalen, vol. cxlvi. p. 161.

a greater density, the membrane will of course be driven inwards, and thus expel the gas and cause the flame to rise quickly. If, on the contrary, the air be suddenly rarefied, the membrane becomes drawn outwards, the space within momentarily increased, the gas expanded, and the flame lowered.

A membrane is known to possess, like every other elastic body, only a definite series of notes; and thus we should suppose that the manometric capsule would only show an effect when the note acting upon it agreed with one of the notes of its membrane.

But this is not the case; for besides the vibration which a body makes under the influence of its elasticity, any motion whatever can be forced upon it if only the active force be much greater than the resistance which it can offer.

For example, let us take a long thin string, tuned to the fundamental note of 100 vibrations, and place its centre in firm connexion with the prong of a strong massive tuning-fork of 110 vibrations; it will then clearly move to and fro 110 times in unison with the vibrations of the tuning-fork, although in accordance with its nature it could only execute 100, 200, 300, &c. vibrations. In point of fact it does not truly vibrate, but is only mechanically drawn to and fro. This is also the case with the manometric capsule, as it is so constructed that the resistance offered to the condensation and rarefaction of the atmosphere must be considered very trifling, indeed almost nil. One and the same capsule is thus equally effective for every note; also different capsules, whose membranes have *not* been tuned in unison, nevertheless give the same results under the influence of the same note.

If out of several capsules which are fed by the same gas-reservoir you set *one* in activity, the flames in all the others are set in motion. Thus, if the membrane be pressed into the capsule, the pressure will not only drive the flame higher from the exit-pipe, but will also spread its influence through the entrance-pipe to the general reservoir, and thence to the other capsules, the flames of which become prolonged, although in a less degree. Of course, a pressure in the contrary direction produces an opposite effect. If, therefore, several capsules are to be employed at the same time, this mutual influence must be annulled.

I at first sought to attain this end by placing between the reservoir and the capsules long thin india-rubber tubes; but this did not act quite satisfactorily.

I attained my object, however, by the use of accessory capsules, through which I permitted the gas to pass before I conducted it into the manometric capsule: they are constructed like the others, each consisting of a cavity closed by a thin membrane.

Fig 7
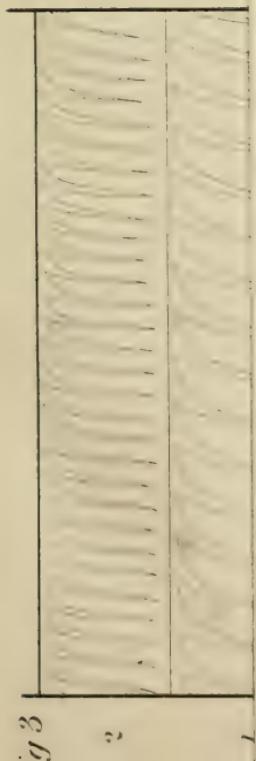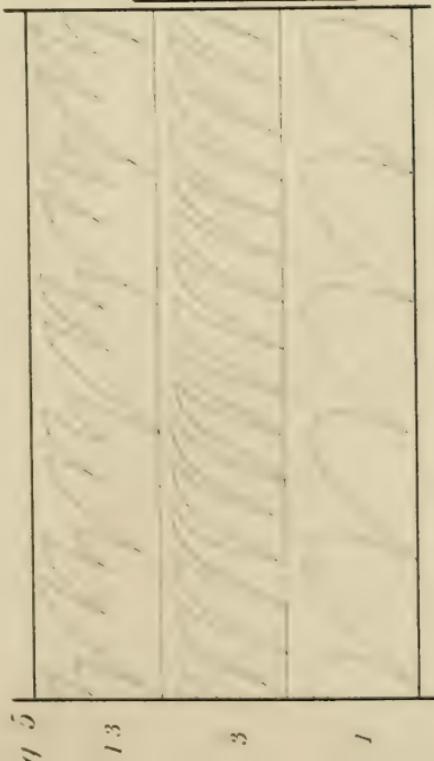
Fig 10.
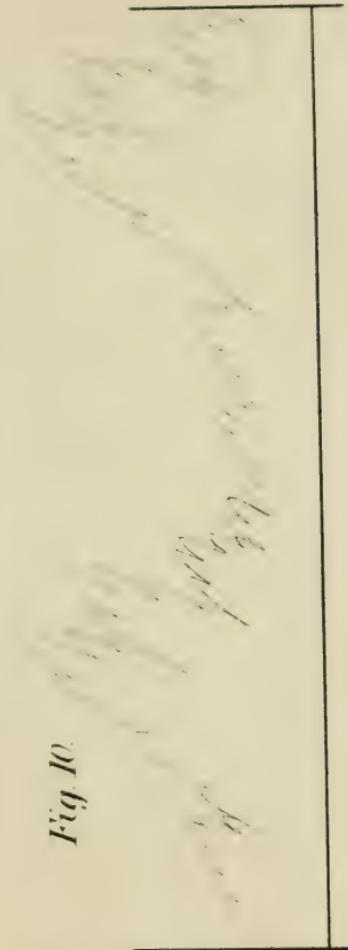
If the pressure derived from the manometric capsule pass through the entrance-tube towards the gas-reservoir, it will be annulled when entering the accessory capsule by the yielding of the membrane.

Practice shows that we may put into the strongest motion one of several flames isolated in the foregoing manner without in any way affecting the rest.

Proof of the different condition of the Air in the Nodes and Ventral Segments of a sounding Air-column.

In order to show the changing density of the air in the nodes and its fixed condition in the ventral segments of a sounding air-column generally, I make use of an open organ-pipe, which is so constructed that either its fundamental tone or its first harmonic or overtone, the octave, can be sounded at will (fig. 1). At the node of the fundamental and the two nodes of the octave are three orifices in one side of the pipe; over these three manometric capsules are so placed that the orifices are exactly closed by the membranes, being of the same diameter; a common reservoir, provided with accessory capsules, feeds the three flames, the length of which can be regulated by cocks.

Fig. 1.

If, now, we give to the three flames an equal height of 15 to 20 millimetres, and sound the octave, then the two exterior flames will be put into such violent motion that they will appear prolonged, narrow, quite blue, and without illuminating power, on account of the considerable amount of air which they draw with them in their flickering up and down, whilst the middle flame will remain almost still and bright, being placed at the centre of a ventral segment, where the air is only gliding to and fro.

At the sounding of the fundamental the middle flame is at the node, and therefore violently agitated; the two exterior ones, which are then between the node and the centres of the ventral segments at the ends of the pipe, show only a weaker motion. As in this case it is only a question of different intensity of motion in the individual flames, it is better here to make use of smaller flames, when the middle one becomes quite blue, while the exterior ones remain bright. If we give the

flames the length of only 8 to 10 millims., on sounding the fundamental the middle flame will be extinguished, on sounding the octave the exterior ones will disappear.

These experiments may also be made with a closed organ-pipe which can be sounded on its fundamental and its first overtone. One of the flames must then be at the end of the pipe, where the node of the fundamental, as well as one of the nodes of the overtone, are found.

If the flame be shortened, on sounding the fundamental the end flame will be the first to go out, and then the middle one, because the latter is nearer to the node than to the ventral segment in the mouth of the pipe.

But on sounding the first overtone, the 12th of the fundamental, the middle flame remains unchanged, while the two exterior ones become extinguished.

Comparison and Combination of several Tones.

These experiments have only shown the general working of whole series of consecutive vibrations; if, however, we allow the flame to be reflected by a rotating mirror, we see all phases of their motion side by side, and we can then not only examine the number of vibrations and the ratios of different tones, but also observe the images made by the combination of several tones.

The apparatus which serves for these investigations consists of a set of organ-pipes, each of which is provided at the node of its fundamental with a manometric capsule. This can be connected by means of an india-rubber tube with gas-burners, which are placed on a special stand (fig. 2).

Before these gas-burners there is placed a revolving mirror, made of four glass plates coated with platinum. The platinum surfaces are turned outwards, in order to avoid the confusing double images of the common mirrors, which is caused by reflection from the two surfaces of the glass plates. A small windchest, for the reception of two organ-pipes, has two mouthpieces, the larger of which serves to conduct the air from a bellows. Through the smaller one the gas is conducted to a common receiver, provided with two cocks, which are joined by means of india-rubber tubing to the capsules of the organ-pipes.

The reflection of a flame at rest shows in the rotating mirror a band of light of the width of the height of the flame. If we, however, sound the organ-pipe in connexion with it, there appears in place of the band of light a series of regularly consecutive flame-pictures, the tips of which are bent in a direction opposite to that in which the mirror is moving.

If we place two burners in such a position that their reflections give two bands of light, one above another, and connect them

with two organ-pipes which together give the interval of the octave, the series corresponding to the higher tone gives double

Fig. 2.
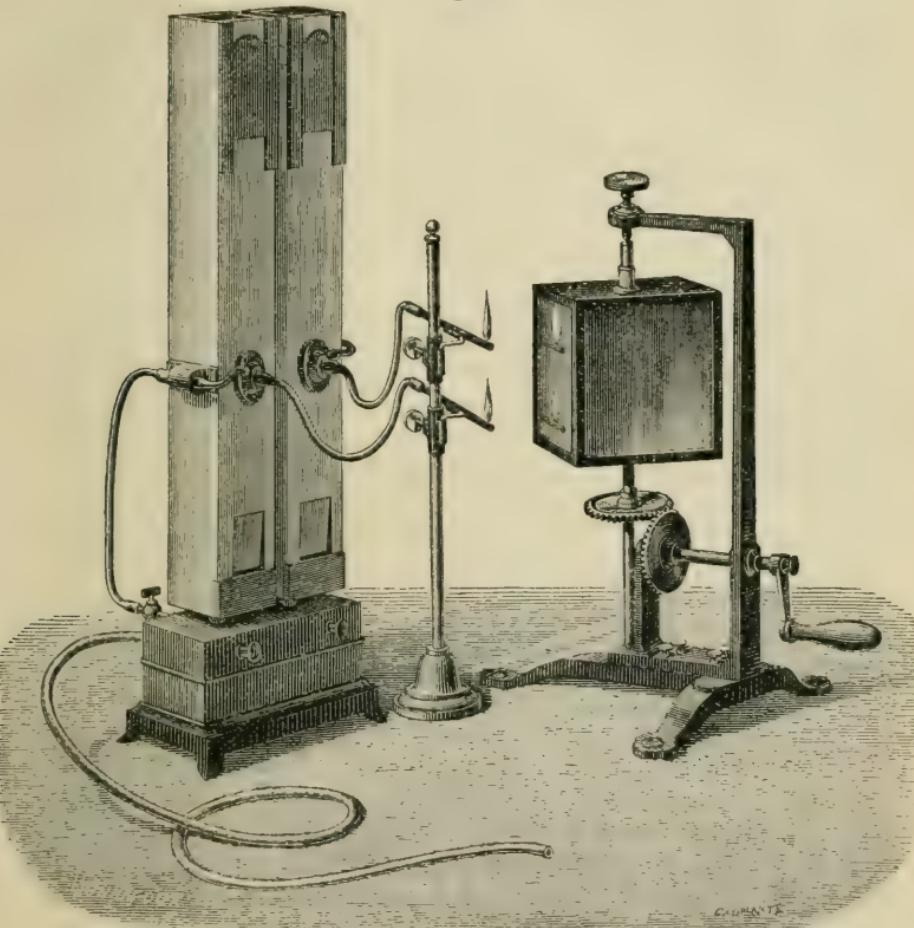
the number of flames that the other one does, by which the vibrations are shown to be in the ratio of 1 : 2 (Pl. I. fig. 3). If we take organ-pipes of other intervals, we get with the fifth three flames above two, with the fourth four above three, and so on.

The rapidity of the motion of the flames allows their reflections in the mirror to be very sharply defined; but as they are of very short duration, it would be difficult in this experiment to observe trifling deviations from the purity of the intervals; for although in point of fact it is easy to recognize that in one of the series there are almost always two flames when there is one in the other, yet it would be difficult to discover that about 200 in the one series coincide with about 101 in the other. These exact observations can be made with the greatest facility, how-

ever, if we make the two capsules of the two corresponding organ-
pipes act on the same flame.

If we sound two organ-pipes, exactly tuned to an octave,
while the gas streams from their two capsules into the same
burner, the flame has the appearance of containing within it a
smaller one without motion. By the slightest discord, how-
ever, the latter becomes flickering, and lengthens and shortens
periodically within the greater one. Each of these double
movements composed of ascending and descending shows a
fluctuation, either the deviation of the upper tone by a
double vibration, or of the lower by a single vibration from the
pure interval of the octave.

The fifth (2 : 3) shows three, the fourth (3 : 4) four, the third
(4 : 5) five points of flame one above another, whose mutual
position remains unchanged with the perfect purity of the in-
terval; on the contrary, any deviation from this causes an up-
and-down movement among them of each single point, which
takes the appearance of a waving motion.

In all these intervals it is easy so to arrange the length of the
flame that all the points may remain clearly bright and appear
separated from each other by blue non-luminous parts of the
flame. If, however, the ratios of vibration of the two tones
becomes more complicated, it is often difficult to observe them
exactly; but even in this case the flame shows plainly whether
the interval be pure or out of tune, as we have but to see whether
the flame is at rest or in motion.

This property of the manometric flame, of showing the least
deviation from the purity of the interval, makes it in many
cases exceedingly useful in tuning, as it is not necessary that
the two notes which are to be brought into tune should be
produced by organ-pipes provided with capsules : the notes of
any instrument may be used, if they are produced before two
resonators in relation with them, which act on two manometric
capsules whose gas-pipes end in the same burner.

The ratio 1 : 2 is the most convenient, on account of its easy
examination ; so that if we want to tune a series of tuning-forks
to the same note, it is better to choose the fork for comparison
an octave lower or higher.

If we wish to observe the whole process of vibration in the
above-mentioned flames on which two notes act at the same
time, we must again employ the rotating mirror. The pure
octave shows in it a series of flames, in which a shorter always
follows a longer, and the shorter ones have all, like the longer
ones, equal heights (fig. 4, Pl. I.). If any *beats* occur, the
summits of the smaller as well as of the larger flames move up
and down. However, these motions are opposed; so that in

those positions where the long flames are at their longest, the short flames are at their shortest, and *vice versâ*.

In fig. 4, Pl. I. the picture of the seventh $(8 : 15,$ or $8 : 16 - 1)$ shows this process, although in a very short period.   The fifth $(2 : 3)$ shows a period of three, the fourth $(3 : 4)$ of four, the third $(4 : 5)$ of five, and the second $(8 : 9)$ of nine in the range of the increasing and then decreasing flames.

If the proportion is not of the form $n : n \pm 1$, then there takes place in the whole period not only a rise and fall of the flame-summits, but the curve connecting them shows as many elevations and depressions as the difference between the two ratios.   For example, see the picture of the sixth $(3 : 5)$ (fig. 4, Pl. I.).

The more complicated the interval of the two notes, the more carefully we must bring it into perfect purity of tune, until no further movement whatever can be discerned in the flame, because otherwise the recurring periods of the flame-pictures in the mirror suffer continual change by the change of phase, and in that case it becomes difficult to recognize them.   But this exact tune becomes still more imperative if we wish to combine more than two notes while making them act on one flame.   It will be remarked, besides, in these experiments, how difficult it is to retain absolutely constant notes with organ-pipes, even when we make use of a well-regulated bellows.

Coexistence of two Tones in the same Air-column.

The investigation of the combination of two related tones in one flame-picture is especially useful, because it teaches us from the flame-picture of a combination of tones, of which the components are unknown, to find the single tones of which it is composed.

A passage to the trial of such a combination of tones as, *e. g.*, each sound offers, is the combination formed by a fundamental with a known overtone in the same air-column.   Very suitable for such an experiment is the above described closed organ-pipe with three flames, since the node of the fundamental as well as one node of the first overtone are situated at their ends.

If we blow the fundamental (1) very gently, the flame-picture in the mirror shows the vibrations of this tone ; if now we blow the overtone (3) strongly, each single vibration will be replaced by three flames.   With a rather weaker blast both tones are produced together, and we always see three flame-summits over every fundamental flame (fig. 5, Plate I.).   Therefore several tones present at the same time in the air-column give exactly the same flame-picture as the combination of the same tones when each is produced by its own particular organ-pipe.

Representation of Sounds.

The apparatus which is used for the representation of sounds consists simply of a manometric capsule, before the membrane of which there is a small cavity terminating in a short tube (fig. 6). The sounds to be represented must be conducted into

Fig. 6.
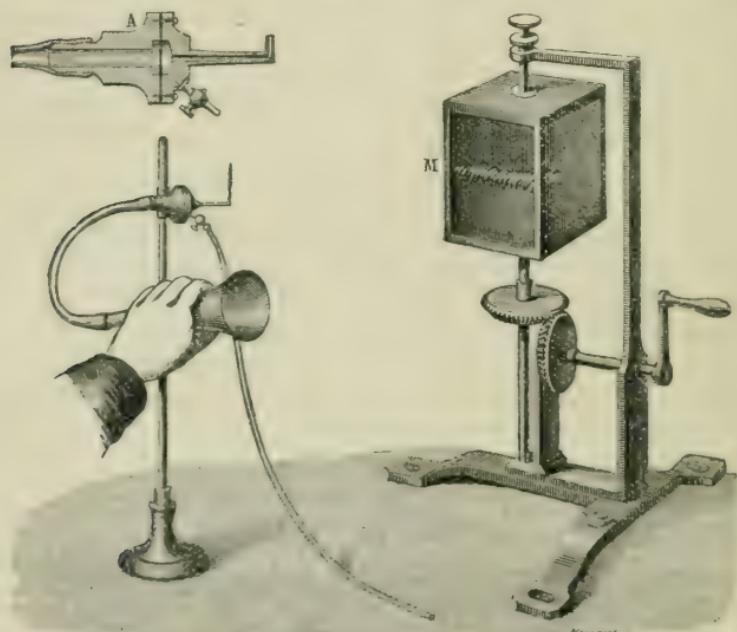
this cavity with the smallest possible loss of their intensity and without undergoing any change in their passage.

The sound-pictures of the combined tones of the same instrument are never all alike, but the deepest tones always show much larger and more complicated flame-groups for each single vibration of the fundamental than the higher ones, because the high harmonic tones, which are to be heard in the sound of the deeper tones of the instrument, disappear more and more as the fundamental ascends. Thus the higher the tone the smaller in comparison are the dimensions of the means which produce it. The vibrations of all resounding instruments, however, take a simpler form if the dimensions of the latter are very small, because the different bodies lose their capacity of forming subdivisions in vibration, by which the accessory tones, if not exclusively, yet in many cases are chiefly produced.

A second reason, however, and that a very potent one, is this. If the tones are produced not so much by the elastic vibrations of a body as rather by gusts of air, as in the siren and pandean

pipes, the upper notes which are contained in the sound of a lower note have so high a place in the scale for a high note that they produce no effect, either on the ear or on an artificial membrane.

The lowest note of the violin, for example, is g (192 vibrations), and its 8th harmonic $\overline{\overline{g}}$ (1536 vibrations) is within the range of the instrument. It is produced on the G-string by a length of 4, and on the E-string by about $13\frac{1}{2}$ centimetres. Nevertheless, if we take this very $\overline{\overline{g}}$ as fundamental tone, the length of string of its eighth harmonic on the E-string would be about 17 millimetres; and besides, with 12,288 vibrations, it would be already nearly two octaves above the highest notes used in music, which sufficiently explains why it is not heard in the sound of $\overline{\overline{g}}$.

My success was but partial in the representation of violin sounds, owing to the high position of the notes of the instrument, since, with the exception of the notes from g to $\overline{c}$ on the G-string, I obtained only the fundamental vibrations for all the rest. In my endeavours to conduct the notes as loud as possible to the membrane I tried two methods. First, I connected the interior air of the violin with the small apparatus, by means of an india-rubber tube which I introduced into one of the f-shaped apertures of the violin; and secondly, I pressed my stethoscope with its concave membrane on the bottom-piece of the violin, precisely under the sounding-post, and attached the india-rubber tube to the flame-apparatus. The results in the latter case were as follows.

On the G-string g showed the figure of the octave in weak wave-formed flames, which, as far as b, rose to sharply defined clearly cut flames. With $\overline{c}$ the latter fell quite suddenly into one single broad, short, and faint flame, in which I could perceive only the smallest trace of the octave when played forcibly. Already the D-string only showed simple flame series, which for $\overline{d}$ $\overline{e}$ $\overline{f}$ $\overline{g}$ were rounded, wave-like, and weak, but on playing $\overline{a}$ became again stronger. The $\overline{a}$ on the A-string gave very high and deeply cut flames, b still stronger ones, which fell, however, at $\overline{\overline{c}}$ and became quite weak. Up to $\overline{\overline{g}}$ and $\overline{\overline{a}}$ on the E-string every trace was lost of the small flame-points which had appeared at the last overtones.

On the connexion of the interior air with the apparatus, the insensibly graduated picture of the octave from g changed into a single sharply defined flame at b; this attained such an extra-

ordinary height at $\bar{c}$, as though it had been produced by the vibrations of an organ-pipe provided with a capsule at the node.

The note $\bar{d}$ also showed a series of high and sharply defined flames, which, however, quite disappeared at $\bar{e}$ to give place to the weak rounded-off waving lines as far as $\bar{a}$.

This sudden appearance of very high flames in the region of $\bar{c}$ is explained by the circumstance that the lowest proper note of the interior air of the violin is precisely $\bar{c}$. For the upper notes I obtained the same result as with the stethoscope; that is to say, the notes $\bar{a}$ and $\bar{b}$ again gave much stronger vibrations than $\bar{e}\,\bar{f}\,\bar{g}$, and than the upper $\bar{\bar{c}}\,\bar{\bar{d}}\,\bar{\bar{e}}$ &c.; so that the second peculiar note of the interior air, or rather of the whole system formed by the violin, seems to be in the region of $\bar{a}$ and $\bar{b}$.

With regard to sound, we have in this case certainly been able only to make evident the transition from the figure of the octave to that of the simple note. The siren shows much better the gradual disappearance of the higher upper note from the musical sounds when their fundamental tone is raised. To this end I intercept the impulse above the open perforated plate by means of an arched aperture which expands into a small tube, and is placed immediately above a part of the apertures so as to permit them to affect the flame, while I cause the rotation of the plate to increase from its lowest to its highest swiftness by increased pressure of the air. The mirror then shows at the lowest notes very large and dense flame-groups; these change towards the middle of the great octave into more clearly defined and deep-slit waves, with at first five, then towards c and d with four flame-points. At g and a the number of the points falls to three, at $\bar{c}$ and $\bar{d}$ to two; and at $\bar{a}$ the last trace of the octave disappears from the sound; after this all the still higher notes only show single flame-pictures.

But the result of this experiment is essentially different if when a sounding-chest is fastened over the perforated plate. It first intensifies the upper harmonics of the sound, then the lower, and lastly the fundamental itself: this causes the flame-groups no longer to become simpler gradually and in accordance with the height of the notes, but to show rather sudden changes alternately rising and disappearing. Thus the sound of a siren, over the perforated plate of which a resonance-box giving the note c was placed, after showing a few complicated and faint pictures when the plate was slowly rotated, produced on reaching the pitch c clearly a large flame in agreement with the fundamental tone: this flame had four summits, derived from

overtone 4, which coincided with the proper note of the sound-ing-chest. On turning the plate more rapidly, the flame-picture became simpler, until at f it became one single flame, so that the overtone 3 must be quite wanting in this sound of the siren. The ascending scale had scarcely passed f when there appeared between each two large flames a small but sharply defined flame, which quickly increased in size, and towards $\overline{c}$ reached nearly the height of the chief flames, where the effect of the resonance-box confirmed the fact of its being the overtone 2 of the sound of the siren. Above $\overline{c}$ the smaller flame leant always more towards the larger one, until at $\overline{a}$ it completely disappeared in it: after this again only single flames appeared (Pl. I. fig. 7).

In order to make the sound in these experiments act strongly on the capsule, I provided the resonance-box with a tube, and placed its interior in direct connexion with the flame-apparatus. These experiments, in which the air-impulses of the siren are prevented from passing immediately into the atmosphere, being compelled to pass through a resonator which remains unchanged for all the fundamental tones of sound, give a visible picture of the process of the formation of vocal sounds; for it is known that the air contained in the cavity of the mouth, when speak-ing or singing the same vowel in different tones, is always tuned to the same note, so that the mouth must act on the air-waves produced in the larynx in the same way as the sounding-chest on the air-impulses of the siren. Nevertheless the series of flame-pictures of the same vowel, sung in the tones of two oc-taves, does not show such sudden changes as might have been expected without closer research.

In order to produce the pictures of the vowels, I sing them into a small funnel-shaped mouthpiece which is connected with the cavity before the membrane by a short india-rubber tube; thus they reach the capsule with great intensity (fig. 6).

I had already in 1867 sketched and had painted the pictures of the vowels u, o, a, e, i sung to the notes of the two octaves from C to $\overline{c}$. I proceeded in the following manner. In order to be sure that I had not changed the character of the vowel in the transition from one tone to another, I first verified the proper note of the mouth with the tuning-fork; then, while I sang into the apparatus, an artist drew the picture which he saw in the mirror. I also drew the same picture independently: and if both our drawings were identical they were looked upon as correct; if, however, there were discrepancies, I repeated the experiment until the error was discovered.

The five finished drawings (Pl. II. fig. 8) were unfortunately

too late for the Exhibition; but I was able to exhibit them at the
Meeting of the Association of Natural Philosophers at Dresden
in 1868. I delayed their publication until now because I wished
to revise them with precision, but was always prevented by the
delicate state of my throat, which did not permit me such
fatiguing experiments. But now, since I can no longer hope
to recover, I have used my best endeavours to make the pictures
correct, and give them forth, not indeed as absolutely perfect,
but as nearly so as it was possible for me to make them.

The drawings themselves are much more difficult than might
be supposed, particularly of the large flame-groups of the deeper
notes, not only on account of the evanescence of the pictures,
but also because the flame-summits do not always follow each
other, but are partly situated beneath one another, so that it
appears as though different flame-groups were intermingled, or
rather pushed partly one before another. But these flames,
whose background, so to say, is formed by other flames, easily
escape observation, particularly if the back ones are not so high,
nor the front ones so low, that the bright summits of the latter
stand out upon the blue lower parts of the former. We can
indeed, by a rapid rotation of the mirror, separate all the summits
from each other; but then the whole group becomes difficult to
observe, on account of its great length and the great bending of
the flames.

However imperfect the drawings may be on account of the
absence of some details, yet they give true pictures of the general
outline as portrayed in the mirror. If, for example, the vowel A
be sung on the note C, the picture shows a group from which a tall
bright flame rises near a smaller very blue one; after these come
a whole mountain of regularly toothed flames. Now it is quite
possible that this ridge has really 9 summits, whilst I have only
drawn 8; for it has sometimes appeared to me that there were
more than that number on days when I produced this very low
note stronger and purer than usual; but this does not change the
character of the whole group, which could never be mistaken for
that of U, O, E, or I sung in the same note. In any case, there-
fore, these pictures appear to me sufficient for the representation
of the great difference in the appearance of the sound of the five
vowels, sung on the same note, as well as to show the manner
of the change of the flame-pictures of the same vowel from one
note to another. But this is the chief point, and indeed all
that can be attained with certainty by the apparatus; for just
on account of its great sensitiveness we must not hope for abso-
lutely correct pictures. The details in the group change most
remarkably, not only when the same vowel is sung in the same
note by different voices, but also when the same voice gives vowel

and note with different intensity. A very slight change in the condition of the voice is sufficient to effect great changes in the flame-pictures. For instance, when my throat is weary, instead of obtaining the picture as drawn of U sung on e, I get only a small flame and two tall broad ones, the last in place of two and two in the picture; and similar simplifications are made in all the flame-groups.

In order to see first what influence may be expected from the fixed notes of the mouth-cavity on the flame-pictures, I will give a general view by drawing for each vowel, sung in each note of the two octaves from C to $\overline{c}$, the harmonic overtone to which the characteristic note approximates, and the number of vibrations by which they differ.

For O, A, and E, I take the characteristic notes (given by Helmholtz) $\overline{b}$, $\overline{\overline{b}}$, and $\overline{\overline{\overline{b}}}$; but, differing from the former opinion of Donders and Helmholtz, I have found for U and I the notes b and $\overline{\overline{\overline{b}}}$; so that the five chief vowels are all an octave distant from each other, and the characteristic note of the lowest vowel, viz. U, unites with the lowest note which it is possible for the mouth to intensify by resonance.

In the definition of these notes there is no question of an absolutely exact number of vibrations. If, for example, I find the most powerful resonance of the mouthpiece for U giving between 220 and 230 vibrations, I may take equally 224 or 225 vibrations as the characteristic note of U. I make this remark here particularly because, in a short address to the Paris Academy (April 25, 1870) on the before-mentioned definitions of the characteristic notes of U and I, I gave as the average simple vibrations for U, O, A, E, I, 225, 450, 900, 1800, and 3600—but after a subsequent revision, the equally correct numbers 224, 448, 896, 1792, and 3584. The former are indeed easier to retain, but they refer to no note in use; whereas the latter numbers show the vibrations of the seventh overtone of $\underline{C}_1$, C, c, $\overline{c}$, and $\overline{\overline{c}}$ ($\overline{c}=256$ vibrations.).

In the following Table the first column contains the vowel, the second the note sung, and the third and fourth the two overtones of the sound of that note between which the characteristic sound of the vowel falls, together with the number of vibrations by which one of these notes is lower and the other higher than the proper note of the mouth-cavity.

7

	C	D	E	F	G	A	B	c	d	e	f	g	a	b	c
	7(d)	6(b)	6(c)	5(b)	5(cis)	4(b)	4(c)	4(d)	3(b)	3(c)	3(d)	3(e)	2(b)	2(c)	
	+112	+64	+128	+64	+170·6	+64	+128	+256	+64	+128	+256	+384	+64	+128	
	−32	−96	−42·6	−128	−42·6	−176	−128	−32	−256	−213·4	−128	−42·6	−416	−384	
	6(a)	5(gis)	5(a)	4(g)	4(a)	3(fis)	3(g)	3(a)	2(e)	2(f)	2(g)	2(a)	1(b)	1(c)	

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	C	D	E	F	G	A	B	c	d	e	f	g	a	b	c
	4(c)	4(d)	3(b)	3(c)	3(d)	3(e)	2(b)	2(c)	2(d)	2(e)	2(f)	2(g)	2(a)	1(b)	1(c)
	+64	+128	+32	+64	+128	+192	+32	+64	+128	+192	+234·6	+320	+405·2	+32	+64
	−64	−16	−128	−107	−64	−22	−208	−192	−160	−128	−106·7	−64	−21·4		
	3(g)	3(a)	2(e)	2(f)	2(g)	2(a)	1(B)	1(c)	1(d)	1(e)	1(f)	1(g)	1(a)		

U 448

28 — 14 — 7 ——— 56 — 28 — 14 ————————————————————

| C | c | ⫶ c | C | c | ⎸ c |

| E 3584 | | | I 7168 | | |

‖(c) 13	‖(b) 12	11	‖(b) 10	‖(b) 9	‖(b) 8	7	‖(b) 6	‖(c) 6	‖(b) 5	‖(cis) 5	‖(b) 4	‖(c) 4
+ 80	+128	+ 85	+128	+128	+128	+224	+128	+256	+128	+341	+128	+256

−14 ———————————————————————————— 7 ———

64	32	86	64	86	112		64	192	85	256	86	352	256
−	−	−	−	−	−		−	−	−	−	−	−	−
‖(a) 12	11	‖(a) 10	‖(a) 9	‖(a) 8	7		‖(a) 6	‖(gis) 5	‖(a) 5	‖(g) 4	‖(a) 4	‖(fis) 3	‖(g) 3

| C | D | E | F | G | A | H | c | d | e | f | g | a | h | ⎸ c |

| A 1792 | | | | | | | | | | | | | | |

The characteristic tone of U approaches therefore to the third overtone of D and E, to the second of A and B, and to the fundamental a and b; and in point of fact in the flame-groups of D and E we can perceive a forking into three, in those of A and B into two chief divisions; while the flame-pictures of a and b show a great preponderance in intensity of the fundamental over the accessory tones.

The characteristic tone of O does not approach any of the overtones of the sounds that are sung (except C) nearer than about half a tone; therefore it has but little effect on the flame-pictures.   At a, where it approaches the second, and at d, where it approaches the third overtone, we perceive clearly the forking into two and three parts respectively; but the more complicated groups of A, F, and D do not show any particular prominence of the '   ~s 4, 5, and 6.   This was to be expected, as the air in the mout.. ~annot strongly vibrate if its pitch, as in this case, differs half a tone from the note already weak when sounded.

The characteristic tone of A approaches no overtone nearer than 16 vibrations, except at C and c, where it coincides with th.   nd 7th overtones.   Nevertheless the pictures of C and c do not show the existence of the 14th and 7th overtones, probably because these notes are so high and weak in the larynx that they cannot vibrate the atmosphere in the mouth sufficiently to act on the flame.

The characteristic tones of the vowels E and I are too high to have any effect on the flame; and thus E sung on $\bar c$ shows but such a picture as does a fundamental distantly accompanied by its octave, instead of a group of seven summits.

I, sung on the same note, shows only a series of simple flames, which seem to indicate a simple tone.   This simplicity of the flame-picture here, however, is only apparent, as in all the pictures of I.

The wide, large, and almost forkless flames shown by the different groups are really mostly whole tufts of flame, which appear somewhat confused when the note is weakly given; but when, on the contrary, it is blown with force, and particularly at the mouthpiece, numerous bright points may be clearly seen, which indicate the presence of very high accessory notes.   It is very fatiguing to sing I, and when pitched low is so difficult that I was compelled to omit the sounds from C to F in the drawings.

I made an experiment to see whether the flame-picture would take any different form if I placed the tube, instead of before the mouth, at the back part of it, and then sang A on f; but, with the exception of increased intensity, the result in both cases was the same.

The whispered vowels had but a slight effect on the flame.

The bands of light in the mirror appeared under their influence like an alternately darker and lighter ribbon with irregular small teeth; and the whole was so cloudy and undefined that I could even discover no difference between the different vowels. The semivowels *m* and *n* gave such similar pictures that I could not distinguish between them. I have sketched them for the notes $\bar{e}$, *g*, *e*, *c* (fig. 9, Pl. I.); deeper notes showed longer, but still misty and undefined periods. Of course in these experiments I was obliged to put my nose instead of my mouth to the instrument. The quivering R, silently pronounced, shows a series of flame-summits of different elevations pretty regularly forked or toothed. In the small rotating mirror, with a plate 15 centims. wide, of which I generally make use, these summits appeared to me to follow each other irregularly. But when I employed a larger one, 40 centims. wide, I perceived the regu᎑᎑᎑᎑ᎏodicity of the whole group, which was repeated four or five times in the width of the mirror. The teeth, which are spread over the whole of the flame-summits, are simply caused by the air-current. Of this we can easily convince ourselves by placing the to᎑ little distance from the gums instead of permitting it ᎑᎑ ᎑᎑ ᎑ate on them, and then expelling the air violently through the narrow aperture. The band of flame then appears serrated, without any individual flame-summits rising above it. But if we intone the R, the picture of the note unites with that of the letter, and there ensues such a confused series of single flames and whole groups of dissimilar height and form, that in the evanescence of the picture it is impossible to decipher them. I have sought to give the character of the voiceless R in fig. 10, Pl. I.

The different characteristics of the voiceless explosives P, T, and K are easily recognized. At P the flame suddenly rises straight up high above the average line, then shows two or three similar *élancements*, which are followed by a few rapidly decreasing ones. Both the high and low chief movements show as at R the indentations caused by the air-current.

The rise of the flame is less sudden at T, neither is it so high; and the deep incisions are wanting, which at P in the commencement show two or three rapid *élancements*. At K, which is articulated further back in the mouth, there is still less a sudden rise of the flame; but the picture begins with a regular rising and falling wave, followed by a few rapidly diminishing ones of the same form. The indentation of the whole picture is the same as in P and T.

If we utter one of these consonants many times in succession while continually turning the mirror, we rarely see the picture well; it is therefore better to place the mirror so that the flame shows in one corner, and with a slight turn must pass over its

whole surface. If we utter the consonant only at the moment of making this movement with the hand, we generally succeed in observing the most interesting part, namely the commencement of the picture.

To pursue these experiments further, perhaps it would be advisable to employ a mirror placed in an oblique axis on which it would be turned, and would then show the flame-picture in a continuous circle instead of broken bands.

The voiceless sibilants F, S, and CH give the same unsatisfactory result as the whispered vowels. I could see nothing defined in the confused dim light-bands.

[To be continued.]

II. *On an Acoustic Pyrometer.* By ALFRED M. MAYER, *Ph.D., Member of the National Academy of Sciences, Professor of Physics in the Stevens Institute of Technology, Hoboken, New Jersey, U. S. A.*[*]

[With a Plate.]

HAVING recently devised an arrangement of apparatus (Pl. III. fig. 7)—which is an instrumental simplification of the method first practised by Zoch (Pogg. *Ann.* vol. cxxviii.)—for measuring the number of acoustic wave-lengths contained in a given tube[†], the idea occurred to me that I could use the method for the determination of the variation in the number of wavelengths contained in this tube caused by a change in the temperature of the air which it contains, and thus succeed in readily determining any temperature to which the tube might be exposed.

The accuracy of this (as far as I know) entirely new method of pyrometry, and the facility of its application, can be judged of by the following discussion.

The formula $V = \sqrt{\dfrac{gh\Delta}{d}(1+at)\,\dfrac{c'}{c}}$ gives the velocity of sound in air of a known temperature. This formula, as is well known, is reduced numerically to $V = 333\sqrt{1 + \cdot 00367\,t}$; in which $V =$ the velocity of sound at the temperature t Centigrade, $333 =$ the velocity of sound, in metres, at 0° C., and $\cdot 00367$ is the coefficient of expansion of air under a constant pressure. We will suppose that we have outside of the furnace whose temperature we would determine, an UT_4 organ-pipe, and that we have placed opposite its mouth an UT_4 resonator, and that tubes

[*] Communicated by the Author.

[†] See my previous paper in the Philosophical Magazine (November 1872), "On a Method of detecting the Phases of Vibration in the Air surrounding a Sounding Body, and thereby measuring directly in the vibrating air the length of its Waves and exploring the form of its Wave-surface."

from the nodal capsule of the pipe and from the resonator lead to contiguous gas-jets placed before the revolving mirror. We will also assume that the air in and around the organ-pipe is at 0° C., and that the serrations of the flames of pipe and resonator are, by means of the manometric flame-micrometer, brought to coincidence when 13 metres of metal tube, connecting the resonator and its manometric capsule, are placed in a furnace which also has the temperature of 0° C. Therefore the length of a wave in the furnace-tube is $\dfrac{333}{512} = 0{\cdot}65$ metre, and it will contain twenty wave-lengths. Now gradually raise the temperature of the furnace to 820° C. As the temperature rises, we shall see the serrations of the resonator-flame gradually slide over those of the organ-pipe flame; and when the temperature has reached 820° C., we shall have observed that the serrations of the resonator-flame have glided over ten times the distance separating the centres of two contiguous serrations of the flame of the organ-pipe; for at 820° C. the air in the furnace-tube will have expanded to four times its volume at 0° C., and therefore

$$\left(\lambda = \frac{333 \sqrt{1 + {\cdot}00367 \times 820}}{512}\right);$$

it will contain half the number of wave-lengths it did when at 0° C.; and the length of one of these waves in the tube will be 1·3 metre.

We will now determine the limit of accuracy of the method by elevating the temperature of the furnace 100°, or to 920° C. At this temperature the velocity of the pulses in the furnace-tube will equal 696·63 metres; and the length of the wave at this velocity will be 1·36 metre. But $1{\cdot}36 - 1{\cdot}3 = 0{\cdot}06$ metre, the difference in wave-length produced by the increase in temperature from 820° to 920°, and sufficient to cause the serrations to be displaced 0·46 of the distance separating the centres of two contiguous serrations of the organ-pipe flame. But by means of the manometric-flame micrometer[*] one tenth of this displacement can be measured; therefore we can measure an increase of 10° in temperature above 820°.

From an examination of the well-established formula for the determination of the velocity of sound, it will be seen that the accuracy of our determinations of furnace-temperature will depend only on the precision of the coefficient ·00367, which is the

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[*] See my previous paper in the Philosophical Magazine, " On a Method of detecting the Phases of Vibration " &c. In this paper I give the credit of the suggestion on which I founded my micrometer to M. Radau, but I find that it is due to Zoch (*Pogg. Ann.* vol. cxxviii.). Radau mentions it in his *l'Acoustique* without giving credit to the inventor.

number arrived at by Magnus and Regnault for the expansion of air under a constant pressure; and this is one of the most reliable constants we have in physics. Hence the accuracy of our measures to 10° C. will be equal to those of the air-thermometer, whose indications at present are necessarily received as our standards of thermometric determinations.

We will now examine the relation existing between temperatures and wave-lengths. I here give two Tables: the first contains the velocities of sound and the wave-lengths of the note ut_4 corresponding to temperatures between 0° C. and 2000° C.; the second, those corresponding to temperatures between 0° C. and −272°·48 C.

Temperatures.	Velocities.	Wave-lengths.
	metres.	metre.
0° C.	333	0·650
100	389·34	0·760
200	438·53	0·856
300	482·72	0·942
400	523·14	1·021
500	560·74	1·095
600	596·03	1·164
700	629·04	1·228
800	660·67	1·290
900	690·77	1·349
1000	719·64	1·405
1100	747·38	1·458
1200	774·19	1·512
1300	799·96	1·562
1400	824·94	1·611
1500	849·35	1·658
1600	872·96	1·705
1700	895·97	1·748
1800	918·41	1·793
1900	940·26	1·836
2000	961·70	1·878
0	333	0·650
− 50	300·86	0·587
−100	265·00	0·517
−150	223·14	0·435
−200	171·79	0·335
−250	95·60	0·186
−272·48	00·00	0·000

These related numbers I have projected into the accompanying curve (Pl. III. fig. 8), whose abscissæ are the temperatures, and whose ordinates are the wave-lengths. This curve, which is the graphical expression of $y = \dfrac{333 \sqrt{1 + ·00367x}}{512}$, is evidently a

parabola, since it has the form $y^2 = ax$; and y will equal 0 when x has receded to the point on the axis of abscissæ equal to $-272°\cdot48$ C., which is "the absolute zero" of temperature.

It is evident that this curve will give the numerical relations between temperatures and the wave-lengths of any note or the velocities of sound in any gas by merely giving different numerical values to the divisions on the axis of ordinates.

It only remains to give the simplest formula for determining the temperature of the furnace in terms of the observed displacement of the resonator-serrations, and of the known number of wave-lengths in the furnace-tube at the temperature t.

Let t = temp. C. of the air in and around the organ-pipe,
t' =   ,,   ,,   the furnace-tube,
v = velocity of sound at temperature t,
v' =   ,,   ,,   ,, t',
l = number of wave-lengths in furnace-tube at temp. t,
d = observed displacement of resonator-serrations by an elevation of temperature $t'-t$;

then $l-d$ will equal the number of wave-lengths in the furnace-tube (allowance made for elongation of tube by heat) at temperature t'. As the velocity of sound in the furnace-tube will be inversely as the number of waves it contains, it follows that

$$v' : v :: l : l-d;$$

hence

$$v' = \frac{vl}{l-d};$$

but

$$v \quad = 333 \sqrt{1 + \cdot00367\,t}, \quad . \quad . \quad . \quad . \quad (1)$$

and

$$v' \quad = 333 \sqrt{1 + \cdot00367\,t'}; \quad . \quad . \quad . \quad (2)$$

hence

$$\frac{vl}{l-d} = 333 \sqrt{1 + \cdot00367\,t'}. \quad . \quad . \quad . \quad (3)$$

Reducing equation (3), we obtain

$$t' = \left(\frac{vl}{20\cdot16(l-d)}\right)^2 - 272\cdot48, \quad . \quad . \quad . \quad (4)$$

which gives t' in terms of v, l, and d. Combining equations (1) and (3), we obtain

$$t' = \frac{272\cdot48\,(2l-d)\,d + tl^2}{(l-d)^2}, \quad . \quad . \quad . \quad . \quad (5)$$

which gives t' in terms of l, d, and t. But as v has to be calcu-

lated in order to obtain l in equation (5), it follows that equation (4) is the simpler and the more readily worked numerically.

If we call T *the absolute temperature* Centigrade, then

$$T = t' + 272\cdot 48,$$

and equation (4) becomes

$$T = \left(\frac{vl}{20\cdot 16(l-d)}\right)^2, \quad . \quad . \quad . \quad . \quad . \quad (6)$$

in which equation the origin of coordinates is at the vertex of the parabola.

This paper is intended only to give a general account of the new method of pyrometry ; and therefore I have not touched on the details of apparatus and experiment.   These I will present in a subsequent communication, in which accounts of actual applications of the method will be given.   I may, however, here remark that if the tube in the furnace is 13 metres long and 0·015 metre in diameter, it can be coiled into a spiral 0·5 metre in diameter, or into two spirals each of 0·25 metre diameter ; or the tube can be bent on itself several times and thus form a compact fascicle of tubing.   Also the tubing should *ascend* from the resonator to the furnace-tube and *descend* from the latter to the manometric capsule, so that the rarefied air in the hot tube cannot enter the tubing outside of the furnace.   If the serrations of the manometric flames are too dim to be readily observed, they can be rendered distinctly visible even in broad daylight by passing the gas through a glass tube containing tow saturated with benzole.   I have thus been able to exhibit these phenomena at night to an audience in a room brightly illuminated with many gas-jets.   We can also make the vibrating flames brilliant by sifting into them the fine scrapings of lead-pencil.

In ascertaining the number of displacements produced by any elevation of temperature, the furnace-tube is slowly moved into the furnace, so that the displacements of the resonator-serrations can be counted as the tube gradually attains the highest temperature, when the serrations become stationary.

October 12, 1872.

The reader is requested to correct the following errata occurring in my paper " On a Method of detecting the Phases of Vibration " &c., Phil. Mag. Nov. 1872 :—

    Page 323, line 10 from bottom, *for* ventral *read* nodal.
      ,,   ,,   1   ,,   *for* in front of *read* behind.

III. *The Chemistry of Sulphuric Acid-manufacture.*
By H. A. Smith*.

IT is astonishing, considering the importance of the subject, how little the chemistry of vitriol-manufacture has been inquired into. If we look over the records of its progress for the last hundred years, as embodied in the specifications of patents, we will find that the fundamental atoms of the structure remain the same, and that it is merely some particular point in the building itself which has been improved or embellished. I propose in this and succeeding papers to make a minute inquiry into the chemistry of this manufacture, and to elucidate, as far as I possibly can, the various laws which determine the combinations and decompositions occurring in the actions, and the causes which prevent these actions taking place.

I shall not in these papers attempt to give, even on a general scale, the various methods which have been used for the production of sulphurous acid, but shall take as the starting-point the use of sulphur for this purpose, the earliest substance from which this gas was obtained, and one which still exists as the purest source of all our vitriol-manufacture.

The subjects treated of in this paper are :—

Sect. 1. *An experimental examination into the causes which determine the action,* inter se, *of the gases in the lead chamber.*

Sect. 2. *The distribution of the gases in the lead chamber* ; and following from this,

An inquiry into the best form of chamber to be used in the manufacture of sulphuric acid.

I hope to treat in another paper of the distribution of heat in the lead chamber, and also of one or two subjects intimately connected with this manufacture.

Section I.—*An experimental examination of the circumstances which determine the action,* inter se, *of the gases in the lead chamber.*

This inquiry was entered into in the hopes of being able to throw some light on the interior economy of the lead chamber, as at present used in the manufacture of sulphuric acid. Although the method ordinarily employed to show the theory of the formation of this acid is a very good one, yet there are numerous points which cannot be shown, and which can only be pointed out through the agency of chemical analysis.

No one has yet attempted to do this minutely ; and our knowledge of the phenomena of alkali-manufacture is in many

* Communicated by the Author.

respects very much behindhand. Experiments have certainly been made, and great successes have been achieved on all sides; but these have tended more to broad generalizations than to exact chemical facts—to the manufacturing, not to the scientific side of the question. The interior of the lead chamber is comparatively an unknown land to us. Lowthian Bell has lately traced the actions occurring in his blast furnace through every stage, from the bottom to the top, in a series of most laborious experiments; but no one has yet done this with the sulphuric acid-chamber. I now venture to hope that this very limited attempt to at least *commence* such an investigation may be of interest, not only to those engaged in the manufacture, but to those who only look at it from a scientific point of view.

In observing the theory of the manufacture of sulphuric acid, there were many points which it struck me would well repay a closer examination. It is well known that when sulphurous acid comes into contact with one of the high oxides of nitrogen, it deprives it of its oxygen, provided the contact takes place in the presence of steam. But there are many causes which prevent this action; and it is not an uncommon thing to see this experiment fail.

If the heat of the vessel in which the combination is to take place be too high, or if it be not high enough, the result will be failure. And so in practice: every manufacturer knows how careful he must be in regulating the amount of steam he throws into his chamber; otherwise he finds a great amount of sulphurous acid escaping into the atmosphere, a larger amount of nitrous fumes in the acid from his Gay-Lussac Tower, and a smaller yield of vitriol.

It is into the laws which regulate the combination of those gases that I wish to inquire; and I have tried to do so, first, by individual experiment, and then by a careful examination of the lead chamber in which the action takes place.

On the Action of Sulphurous Acid Gas upon Nitric Acid Gas.

Although we are indebted to the labours of Clément, Désormes, Davy, De la Provostaye, and others for the light which has been thrown upon the theory of this action, so far as it relates to sulphuric acid-manufacture, yet I trust I may be excused for bringing forward results which differ in some degree from those of the above-mentioned workers.

It is generally understood at the present day that no action can take place between *dry* sulphurous acid and nitric acid gases when brought together in the same vessel; and in all

chemical treatises this fact is distinctly stated. Thus Miller, in his 'Elements of Chemistry,' speaking of the theory of the manufacture of sulphuric acid, says :—" *Direct combination, however, cannot be produced between the two gases* (oxygen and sulphurous acid) ; *the intervention of a third substance becomes necessary, and if water be presented to them a very gradual process of oxidation occurs.*"

Gmelin also, in his ' Handbook of Chemistry,' observes :— " A *dry* mixture of two measures of sulphurous acid gas and one measure of oxygen *remains unaltered* ; but if water be present, a very gradual condensation takes place, and sulphuric acid is produced." The results arrived at by different observers may be summed up in the deduction from the above paragraphs— namely, *That no action can take place between these two gases without the intervention of water, either in the liquid or gaseous state.*

It was with the intention of inquiring more closely into this that the following experiments were made ; and the conclusion I come to, and which I hope to clearly demonstrate in this paper, is the very reverse of that generally admitted—*That action does take place between the dry gases under certain conditions.*

As I was anxious to have the gases in as similar a condition as possible to those in the lead chamber, the sulphurous acid was made from burning sulphur, the nitric acid being prepared from nitrate of soda by the action of sulphuric acid, whilst the air employed was first carefully dried by passing through sulphuric acid and caustic potash, every care being taken to prevent the presence of even the smallest amount of moisture.

Exp. I. When dry sulphurous acid and nitric acid in the gaseous form are brought into contact in a perfectly dry glass vessel, which is then hermetically sealed, there is *apparently* no action (for this experiment I used a vessel of the following shape) ; but if this mixture, after being allowed to stand for ten or twelve days, be then opened, and the remaining gases expelled, it is found that a decided, though small amount of sulphuric acid has been formed over the sides of the glass vessel, and may be seen in the condition of white crystals, soluble in water, and behaving in all respects as sulphuric acid. There are many things which will prevent this formation, and which I intend noticing further on, such as temperature &c. This result, however, is greatly hastened by the addition of *a single drop* of water on the end of a fine platinum wire.

The conclusion one was apt to arrive at was that these crystals were merely the ordinary chamber crystals ; but I was led

to doubt this from various circumstances. In the first place, they seemed to differ from the chamber crystals in *form*, these partaking more of the needle-shaped form of the crystals of sulphuric acid ; and also, when exposed to the air, they remained a long time (several days in fact) without change. This shows they are not the same as the chamber crystals. Again, when they were brought into contact with water, they dissolved without the evolution of nitrous fumes, from which we may fairly conclude that they were crystals of sulphuric anhydride.

I had long been inclined to take a different view of the cause of this action from that generally accepted. It seemed to me, in all its phases, to resemble the action of a small piece of leaven in a loaf of unleavened bread, or the action of a minute crystal dropped into a supersaturated solution of a salt, which immediately causes the solidification of the whole liquid. The action only requires to be commenced, and it then continues till the whole of the attainable oxygen has been made use of. This action, if quickened at all, is only slightly so, by the further addition of steam.

Exp. II. If, again, instead of inserting a drop of water into the vessel in which the gases are confined, it be surrounded with a coating of ice, the same effect takes place, a much longer time, however, being required.

Exp. III. The same end can also be attained by the *sudden* application of heat.

These results then led me to judge of the action as I have said, showing that the sulphurous acid is able to deprive the nitric acid of some of its oxygen without the intervention of steam, a medium which has hitherto been considered necessary.

It has been shown that a very small amount of water can cause the action to commence. The next experiment tried was to find what effect water present in a large quantity had upon the formation of the acid.

Exp. IV. A mixture of two volumes of steam to one of the mixed gases was put into a glass vessel and allowed to stand twenty-four hours (the gases being mixed in requisite proportion). The same amount

(*Exp*. V.) of the mixed gases was passed into a similar vessel, but into which *no air* was allowed to enter, to see if any action could take place in its absence ; and the water was presented to the gases in the liquid condition on a platinum wire, and allowed to stand the same length of time. At the end of the time the results were :—

Acid produced, cal-
culated to percentage.

Exp. IV. . . . 66 per cent.
Exp. V. 93   ,,

a certain amount of nitric acid still remaining untouched. In
both cases, however, the result, *in appearance,* was a complete
combination. Still, on examination, a large amount of nitric
acid was found in solution in water in No. 1 (Exp. IV).

Exp. VI. Equal volumes of steam and mixed gases were then
tried, and the result on examination gave—

Acid produced, cal-
culated to percentage.

Exp. VI. . . . 74 per cent.

From these experiments, then, I considered myself justified
in adopting the theory of the leavening nature of the action
taking place.

It is also evident that the volume of steam introduced should
be less than the combined volumes of the two gases. But let
us take another case. Suppose the temperature of the vessel in
which the experiment is to take place be raised to 100° C., or
kept in boiling water, the results are found to differ in every
case.

Exp. VII., VIII., IX. Taking the same volumes as above,
I find the different yields of acid to be thus :—

Exp.	Acid produced, calcu- lated to percentage.	Exp. above.
VII.	86·7 corresponding to	IV.
VIII. . . .	24·5 ,, ,,	V.
IX.	80·2 ,, ,,	VI.

Here, then, is quite another phase opened up to us : tempera-
ture has a great deal to do with the action taking place ; and,
as the result of many experiments, I find that its influence may
be embodied in the general rule, that " *The higher the tempera-
ture the more steam required.*"

The foregoing results thus show the action occurring when
nitric acid gas is brought into contact with sulphurous acid gas,
both with and without the presence of steam. The next question
to be solved is, In what part of the vessel does most action
take place ?

Most persons who have attended chemical lectures are familiar
with the method employed to illustrate the formation of sul-
phuric acid. The mixed gases of sulphurous and nitric acids

are brought together in a large globe, and steam is then intro-
duced. Ruddy fumes are first formed ; then a crystalline deposit
takes place ; and soon the atmosphere in the globe becomes
white in colour.   But on close examination another peculiarity is
seen in the vessel ; and that is, that near to the deposit, and long
after the ruddiness has apparently disappeared, a very small
narrow band of red fumes is noticed wherever the crystallization
has taken place.   This, then, led to the question being asked,
Does not a greater formation of acid take place when some acid
previously formed is present ? and this question was attempted
to be answered by the following experiments.

Exp. X. A layer of sulphuric acid from which all moisture
had been expelled by long boiling, and which had been pre-
viously carefully weighed, was laid in the bottom of the vessel,
and the gases allowed to enter, steam being excluded.   The gas
at the upper part of the vessel became almost immediately
nearly white ; but a strong and long-continued action seemed to
be taking place at the bottom, near the surface of the sulphuric
acid, no apparent action being noticed towards the top.   On
examination no sulphurous acid was found, whilst the weight of
acid originally present had greatly increased. (In this experiment,
as in the former, the vessel was allowed to stand twenty-four
hours.)   This seemed to answer the question, especially as on
many repetitions the same results were obtained.

I felt these perfectly satisfactory as laboratory experiments.
Some had now to be attempted on a large scale ; and through
the kindness of an acid-manufacturer I was enabled to make
the desired trials ; but as in every case I sustained signal defeat,
being neither able to cool the chamber sufficiently nor raise it
to the required temperature, I found I must, after many dis-
appointments, rest satisfied (as many have found themselves
obliged to do) with a knowledge of the fact that what appears
perfect in the laboratory will not bear the crucial test of manu-
facture !

I still felt satisfied that I had proved the drawback to this to
be want of control over the unwieldiness of the chamber.   But
one fact came out very strongly—that if I wished a good yield of
acid, *the increase of steam must be in proportion to the increase of
temperature.*

The point which next claimed my attention on the manufac-
turing scale was the question, In what part of the chamber
does the greatest formation of acid take place ?

This investigation resolves itself into two separate ones :—

1. The distribution of gases in the lead chamber.

2. The distribution of heat in the lead chamber.

The answer to the first of these embodies almost necessarily

the much vexed question regarding the best form of chamber for use in the manufacture of sulphuric acid.

Section II. *On the Distribution of Gases in the Lead Chamber, and also an inquiry into the best form of chamber to be used in the manufacture of Sulphuric Acid.*

In the preceding page the following observation is made when speaking of the laboratory method of showing the theory of sulphuric acid-manufacture :—" Near to the deposit, and long after the ruddiness has apparently disappeared, a very small narrow band of red fumes is noticed wherever the crystallization has taken place." This, then, when taken into consideration along with exp. X. in the same section, tended to lead me to the belief that the greatest amount of condensation takes place at the bottom of the chamber, near the surface of any sulphuric acid which has been already formed, and that the upper portion of the chamber is of use principally as a reservoir for the gases ; so that if, instead of having a long *high* chamber, one that was long but of low height were to be used, the same purpose would be answered to a greater degree, and the expense of chamber-building greatly reduced. It was with the intention of proving or disproving the truth of this theory that the following investigation was undertaken. The size of chamber used was about 140 feet in length by 30 high and 25 wide. The gases were introduced at the end of the chamber through an iron pipe 12 feet long by $3\frac{1}{2}$ in diameter—the chamber-draft very moderate—steam injected at three points in the side and along with the gases at the end. In order to have a definite plan of proceeding, I took specimens of chamber air at every 10 feet along its length, 15 feet from the bottom—and also specimens at the same distances at 3 feet from the bottom,—thus having

14 analyses, chamber air at 15 feet

and

14   ,,   ,,   3 ,,

the first being in reality a bisection of the chamber along its length.

The gases of which the percentages were obtained were sulphurous, sulphuric, and nitric acids.

Before giving my own results, I should be glad if I could quote those of any other observer ; but I am unaware of any work having been done on this subject ; so that the following observations may be interesting from their newness.

Sulphurous Acid.

Taking, then, in the first place, sulphurous acid, I will show first its distribution in the chamber, then take the two others in the order given above.

As is naturally expected, the largest amount of sulphurous acid is present at the entrance to the chamber; but dispersion takes place very rapidly indeed, so that each 10 feet makes a decided change in the percentage of acid present.

The amount of sulphurous acid present at a distance of 10 feet from end of chamber is equal to 72 per cent., continuing the same till about 20 feet from end. There is then a rapid fall to 46 per cent. at 30 feet, falling rapidly still, till at 40 feet from end the amount only equals 31 to 33 per cent. After this the variations are not so remarkable; the amount of sulphurous acid, however, becomes gradually less, and less till its lowest point is reached at 120 feet from entrance, when 13 per cent. is the amount present. These analyses were made at 15 feet from bottom of chamber. In those made at 3 feet from bottom the variations are not so sudden. At 10 feet from entrance the amount present is 3 per cent.; this rises rapidly, till at 40 feet from end it attains its maximum and 29 per cent. of acid. It is now at its highest point; and from this it begins to descend very gradually, till at 130 feet it falls to 8 per cent., and near its exit rises to 16 per cent.

The following diagrams will show this variation more plainly. The numbers along the top indicate the length of the chamber divided into distances of 10 feet each; those down the side of Diagram I *a* the percentage of acid present.

Diagram I *b*.

Length of chamber in feet.

(Entrance.)	10	20	30	40	50	60	70	80	90	100	110	120	130	140	(Exit.)
feet in height	p. c. 72	p. c. 70 to 72	p. c. 46	p. c. 31 to 38	p. c. 25	p. c. 26	p. c. 30	p. c. 22	p. c. 29 to 30	p. c. 22	p. c. 23	p. c. 13	p. c. 18	p. c. 18	15 feet in height.
eet in height	p. c. 3	p. c. 8	p. c. 16	p. c. 29	p. c. 28	p. c. 18	p. c. 19	p. c. 20	p. c. 17	p. c. 17	p. c. 14	p. c. 13	p. c. 8	p. c. 16	3 feet in height.
(Entrance.)	10	20	30	40	50	60	70	80	90	100	110	120	130	140	(Exit.)

Length of chamber in feet.

The figures in the divisions of the above diagram represent the percentages of sulphurous acid corresponding to diagram I *a*.

The numbers "3 per cent." &c. represent the percentage of acid at 3 feet from bottom of chamber.

The numbers "72 per cent." &c. represent the percentage of acid at 15 feet from bottom of chamber.

Diagram I *a*.
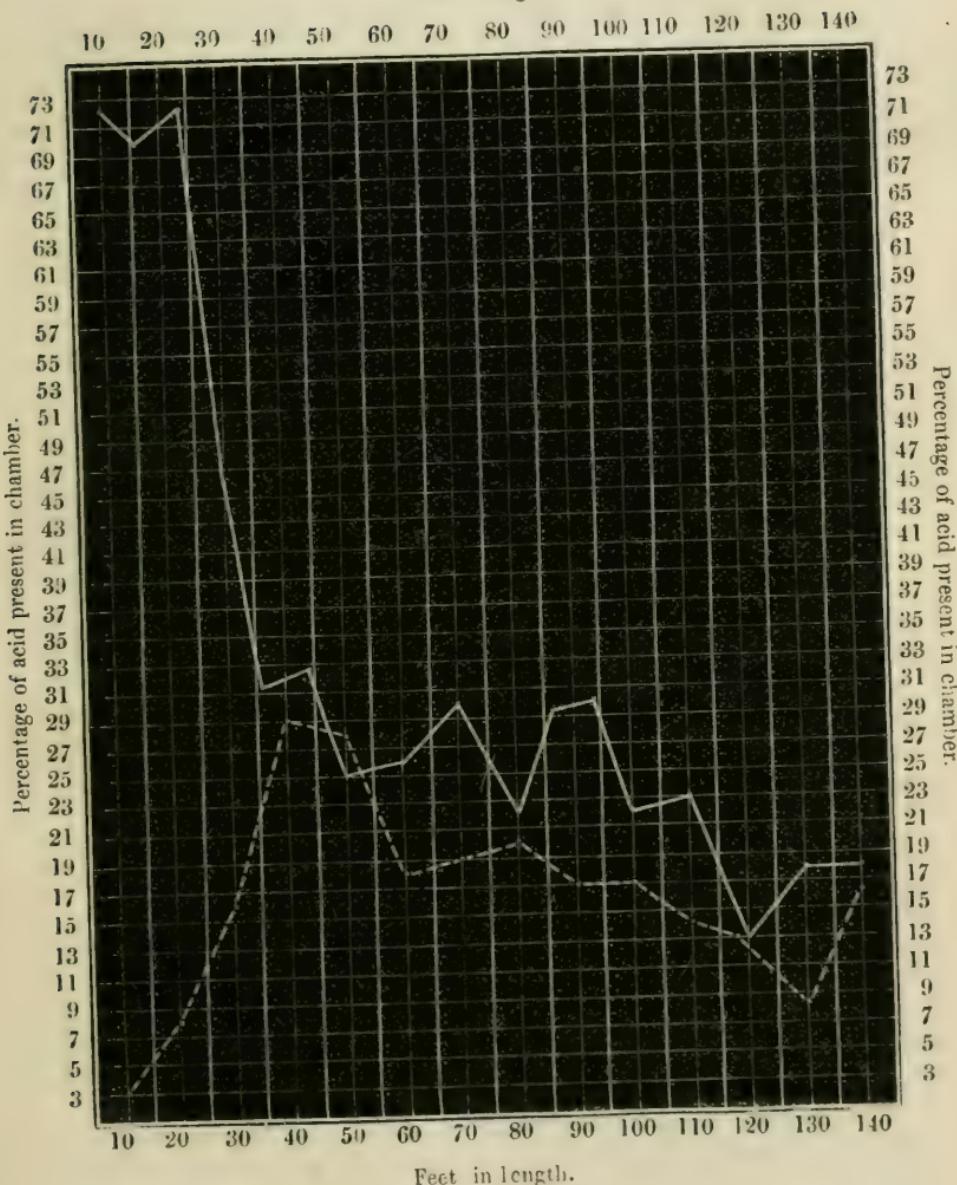
In this diagram the dotted line represents the amount of acid at 3 feet from bottom of chamber.

The white line represents the amount of acid at 15 feet from bottom of chamber.

Or if we take the following Proportional Tables, we shall see the variations (numerical) more plainly.

No. I. At 15 feet height.   No. II. At 3 feet height.

Feet from end of chamber.   Feet from end of chamber.

	No. I		No. II
120	$=1$	10	$=1$
$\left.\begin{matrix}130\\140\end{matrix}\right\}$	$=1\cdot4$	$\left.\begin{matrix}20\\130\end{matrix}\right\}$	$=2\cdot6$
$\left.\begin{matrix}80\\100\end{matrix}\right\}$	$=1\cdot7$	120	$=4\cdot3$
110	$=1\cdot8$	110	$=4\cdot6$
50	$=1\cdot9$	$\left.\begin{matrix}30\\140\end{matrix}\right\}$	$=5\cdot3$
60	$=2$	$\left.\begin{matrix}90\\100\end{matrix}\right\}$	$=5\cdot6$
$\left.\begin{matrix}70\\90\end{matrix}\right\}$	$=2\cdot3$	60	$=6$
40	$=2\cdot5$	70	$=6\cdot3$
30	$=3\cdot5$	80	$=6\cdot6$
$\left.\begin{matrix}20\\10\end{matrix}\right\}$	$=5\cdot5$	50	$=9\cdot3$
		40	$=9\cdot6$

In looking over Diagram I *a*, it may be noticed that there are three distinct falls in the percentage of acid, and after each there is again a slight rise. It may be interesting to remark that almost exactly at those parts were the points at which steam was thrown into the chamber. The falls are at 20 feet, 70 feet, and 110 feet respectively, steam being injected at 20 feet, 65 feet, and 110 feet.

The tremendous fall occurring from 20 feet to 40 feet may be accounted for by the great amount of steam entering the chamber at this point—as not only was it entering at 20 feet, but also, along with the gases, at a little below the large iron pipe at the end of the chamber; so that the steam absorbs a large amount of the hot sulphurous acid.

In the analyses at 3 feet these falls are not so noticeable, are indeed not so great, the acid here being out of the immediate action of the steam. In these experiments the temperature of the chamber was kept as low as possible, and the amount of steam allowed to go into the chamber was, as far as could be determined, almost one quarter the volume of the mixed gases. This, then, tends so far to show, what I previously imagined, that the upper portion of the chamber was of use principally as a reservoir for the sulphurous acid, allowing it to descend as it was required to the lower or working portion. This was also tried in another way.

The ordinary funnel-shaped collector usually placed in the chamber, and which communicates with a small leaden jar on the outside, by which a manufacturer gets an idea of the strength and make of his acid, was brought into use. Instead of being, as usual, placed about 8 feet from the bottom of the chamber, it was in this case placed about 16 feet high, and the amount of sulphuric acid formed was carefully observed.

In this case, after standing nine days, only $\frac{1}{16}$ of an inch of acid had formed, whilst at the height of 4 feet the make of acid was regular and fairly large.

In the former case (at 16 feet) sulphurous acid was continuously escaping, whilst the amount in the latter was merely trifling. This, then, was a very fair proof of the truth of my theory.

Sulphuric Acid.

Passing from the sulphurous to the sulphuric acid, I find the diagram of percentage in the latter a very strange one. At 10 feet from the point of entrance, where 0 is the percentage of acid, to 140 feet the variation at 15 feet in height is very trifling. The highest amount is reached at 50 feet, showing there only 23 per cent. But at 3 feet the analyses present a more extraordinary difference than that of Diagram I.

Beginning again at 10 feet from entrance, I find the amount of acid to be equal to 81 per cent.; then a sudden rise brings it to 89 per cent., this being the maximum; then comes a most rapid and continuous fall, till at 100 feet the amount is 30 per cent., and remains nearly at this to the end of the chamber.

The following diagrams will show the variations.

Diagram II*b*.

Length of chamber, in feet.

(Entrance.)	10	20	30	40	50	60	70	80	90	100	110	120	130	140	(Exit.)
15 feet in height.	p. c. 0	p. c. 0	p. c. 6	p. c. 18	p. c. 23	p. c. 20	p. c. 18	p. c. 16	p. c. 19	p. c. 12	p. c. 12	p. c. 7	p. c. 7	p. c. 10	15 feet in height.
3 feet in height.	p. c. 81	p. c. 89	p. c. 76	p. c. 70	p. c. 68	p. c. 67	p. c. 60	p. c. 56	p. c. 48	p. c. 30	p. c. 38	p. c. 30	p. c. 36	p. c. 33	3 feet in height.
(Entrance.)	10	20	30	40	50	60	70	80	90	100	110	120	130	140	(Exit.)

Length of chamber, in feet.

The figures in the divisions of the above diagram represent

Diagram II a.

Feet in length.

Feet in length.
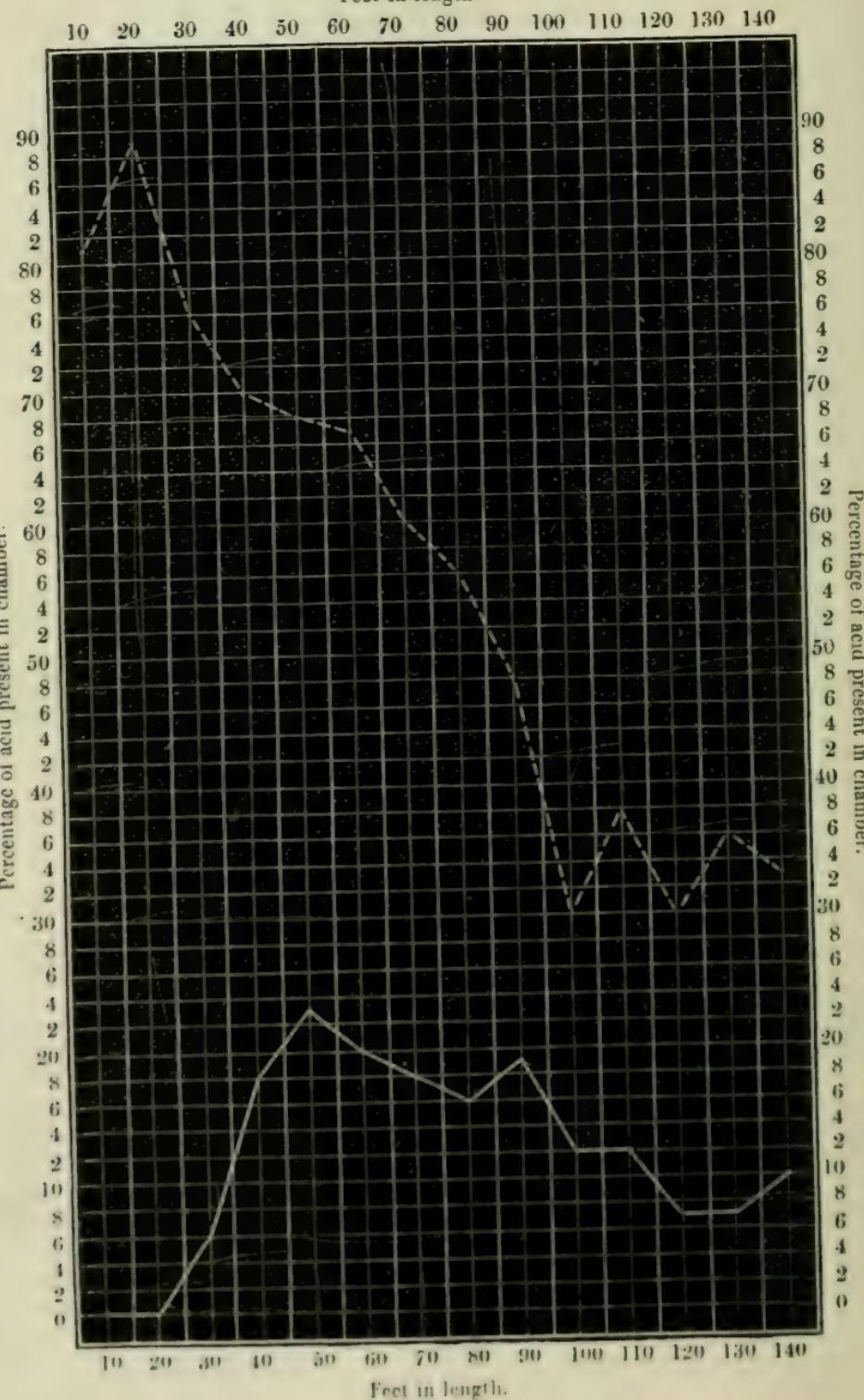
the percentages of sulphuric acid, corresponding to the lines in Diagram II *a.*

The numbers "0 per cent." &c. represent the percentage of acid at 15 feet from bottom of chamber.

The numbers "81 per cent." &c. represent the percentage of acid at 3 feet from bottom of chamber.

In Diagram II *a,* as in Diagram I *a,* the top figures denote the length of the chamber divided into spaces of 10 feet each, whilst the side figures denote the percentage of acid present.

The dotted line represents the amount of acid at 3 feet from bottom of chamber.

The white line represents the amount of acid at 15 feet from bottom of chamber.

On comparing this with Diagram I *a,* the similarity is very striking. It is only required to place the white line in Diagram I. on the dotted line in Diagram II., and *vice versá.* In No. I. the greatest amount of sulphurous acid is at the top of the chamber, and the smallest amount at the bottom. In No. II. the largest amount of sulphuric acid is at the bottom of the chamber, whilst the smallest amount is at the top.

Proportional Tables for Diagrams II.

No. I. At 15 feet height.	No. II. At 3 feet height.
Feet from end of chamber.	Feet from end of chamber.
30 $= 1$	$\left.\begin{array}{l}100 \\ 120\end{array}\right\} = 1$
$\left.\begin{array}{l}120 \\ 130\end{array}\right\} = 1 \cdot 2$	140 $= 1 \cdot 1$
140 $= 1 \cdot 7$	$\left.\begin{array}{l}110 \\ 130\end{array}\right\} = 1 \cdot 2$
$\left.\begin{array}{l}100 \\ 110\end{array}\right\} = 2$	90 $= 1 \cdot 6$
80 $= 2 \cdot 7$	80 $= 1 \cdot 8$
$\left.\begin{array}{l}40 \\ 70\end{array}\right\} = 3$	70 $= 2$
90 $= 3 \cdot 2$	$\left.\begin{array}{l}60 \\ 50\end{array}\right\} = 2 \cdot 2$
60 $= 3 \cdot 3$	40 $= 2 \cdot 3$
50 $= 3 \cdot 7$	30 $= 2 \cdot 5$
$\left.\begin{array}{l}10 \\ 20\end{array}\right\} = 0$	10 $= 2 \cdot 7$
	20 $= 2 \cdot 9$

Nitric Acid.

The variations in the percentage of nitric acid are not very great, being only between 3 per cent. and 26 per cent., these being the maximum and minimum amounts. It attains its greatest height at 100 and 110 feet from end of chamber, and then sinks very rapidly down to 3 per cent. at 140 feet.

The figures and lines are similar to those in the preceding diagrams.

The dotted line represents the amount of acid at 3 feet from bottom of chamber.

The white line represents the amount of acid at 15 feet from bottom of chamber.

Diagram III *a*.
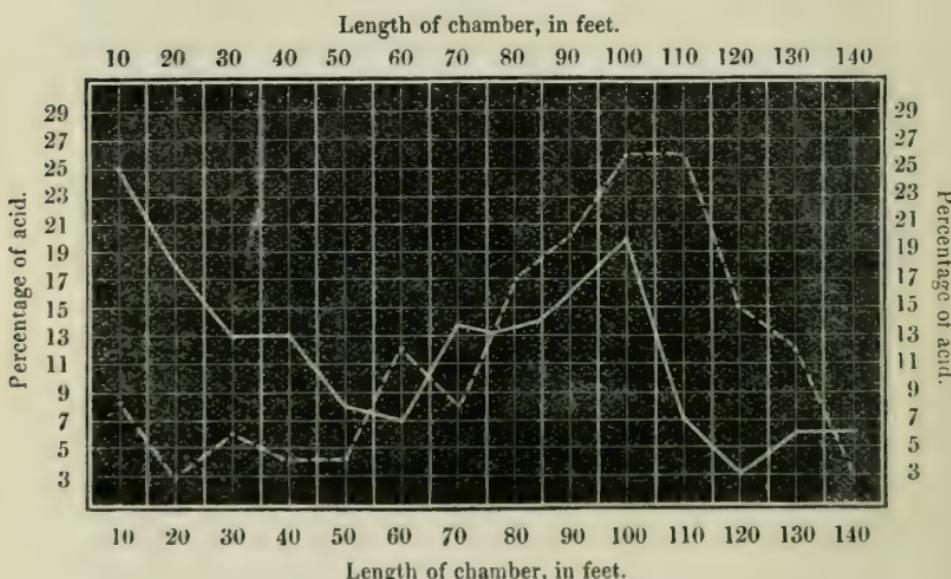
Length of chamber, in feet.

Diagram III *b*.

Length of chamber, in feet.

(Entrance.)	10	20	30	40	50	60	70	80	90	100	110	120	130	140	(Exit.)
15 feet in height.	p. c. 25	p. c. 18	p. c. 13	p. c. 13	p. c. 8	p. c. 7	p. c. 14	p. c. 13 to 14	p. c. 16	p. c. 20	p. c. 7	p. c. 3	p. c. 6	p. c. 6	15 feet in height.
3 feet in height.	p. c. 8	p. c. 3	p. c. 6	p. c. 4	p. c. 4	p. c. 12	p. c. 8	p. c. 17	p. c. 20	p. c. 26	p. c. 26	p. c. 15	p. c. 12	p. c. 3	3 feet in height.
(Entrance.)	10	20	30	40	50	60	70	80	90	100	110	120	130	140	(Exit.)

Length of chamber, in feet.
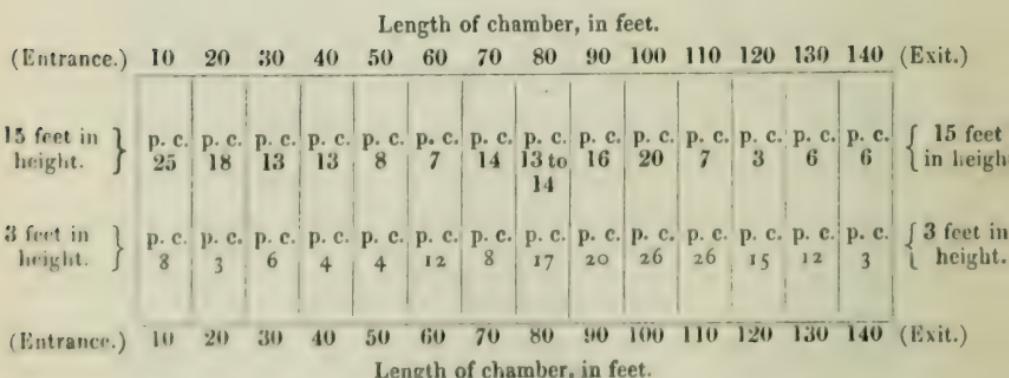
The figures in the divisions of the above diagram represent the percentages of nitric acid, corresponding to the lines in Diagram III *a*.

The numbers "8 per cent." &c. represent the percentage of acid at 3 feet from bottom of chamber.

The numbers "25 per cent." &c. represent the percentage of acid at 15 feet from bottom of chamber.

I also give the following Proportional Tables for Diagrams III.

No. I. At 15 feet height.	No. II. At 3 feet height.
120　 = 1	20 ⎫ = 1
130 ⎫ = 2	140 ⎭
140 ⎭	40 ⎫ = 1·3
60 ⎫ = 2·3	50 ⎭
110 ⎭	30　 = 2
50　 = 2·7	10 ⎫ = 2·7
30 ⎫ = 4·3	70 ⎭
40 ⎭	60 ⎫ = 4
70 ⎫ = 4·7	130 ⎭
80 ⎭	120　 = 5
90　 = 5·3	80　 = 5·7
20　 = 6	90　 = 6·7
100　 = 6·7	100 ⎫ = 8·7
10　 = 8·3	110 ⎭

We have now before us results which may assist us in coming to some conclusion regarding the most useful form of chamber. We have seen that the chamber must be divided into two parts —the working portion, and the reservoir (so to speak) for the gases. If, then, instead of employing this reservoir we lowered the height of the chamber and extended its length, we should have a greater condensing surface, as we have seen that the greater amount of acid condenses near the surface of already formed sulphuric acid. We should also have a form of chamber better adapted for getting a good draught. It would not be at all difficult for manufacturers to have a Table drawn up for the manager of their chambers, showing him the amounts of steam necessary to be thrown into the chamber according to the increase or diminution of temperature; they would thus have a great saving, both in the amount of gas escaping useless to the chimney, and also in the amount of acid manufactured. I hope soon to show the temperature at which the greatest amount of action takes place between the gases in the chamber, but would merely mention that it is of the greatest consequence to the manufacturer to take particular notice of the temperatures of his chamber, as upon the successful management of this depends in a very great degree the yield of acid, and also that trouble to all manufacturers, the flowering of the sulphur in the acid.

[To be continued.]

IV. *On the Definition of Intensity in the Theories of Light and Sound. By* Robert Moon, *M.A., Honorary Fellow of Queen's College, Cambridge**.

MR. BOSANQUET appears to think† that in my paper in the Philosophical Magazine for October last, I have fallen into an error in estimating the *vis viva*.

In point of fact I never attempted to estimate the *vis viva*. Adopting the definition of intensity propounded by three out of the five writers to whom I referred, viz. that the intensity is measured by the square of the amplitude, I pointed out that, admitting the square of the amplitude properly to represent the effect on the eye or ear of a single undulation, we must divide that quantity by the time—or, as Mr. Bosanquet would express it, we must multiply it by the number of undulations incident on the organ in a unit of time—in order to arrive at the true measure of intensity of the ray or note.

I must plead guilty, however, to having overlooked the contrariety exhibited by the definitions cited in my paper,—Sir John Herschel, Dr. Lloyd, and Mr. Airy taking the square of the amplitude as the measure of intensity, while Prof. Tyndall and Dr. Helmholtz (herein following Fresnel) adhere to the square of the maximum velocity as the measure; which latter, as Mr. Bosanquet points out, will have the square of the periodic time in the denominator, assuming the vibration to be correctly represented by the formula ordinarily employed for that purpose‡. Having always worked with the former definition, and never having heard that there was any dispute about the matter, I took for granted without inquiry the identity of the definitions, although the slightest examination would have shown them to be irreconcilable§.

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* Communicated by the Author.

† See Phil. Mag. S. 4. vol. xliv. p. 386.

‡ That in some important particulars the formula completely misrepresents the vibration is certain. I do not dwell upon this, however, as the want of correspondence between the two definitions is abundantly obvious.

§ Sir John Herschel adopts both definitions, apparently without any consciousness of their incongruity; for, while in the passage I have quoted from his 'Treatise on Light' (No. 563) he speaks of the amplitude as determining the intensity both of light and sound, in his 'Treatise on Sound' (No. 126) he gives the following:—

"In the theory of sound, as in that of light, the intensity of the impression made on our organs is estimated by the shock, impetus, or *vis viva* of the impinging molecules, which is as the square of their velocity—and not by their inertia, which is as the velocity simply."

I may remark that the notion of the intensity of our sensations being measured by the "shock" of the "impinging molecules," which Sir John Herschel here adopts from Fresnel, is founded on a complete misappre-

But this alternative definition, just as much as the former, stands in need of correction. For, any claim which the maximum velocity can have to be regarded as the test of the intensity of a note or ray must rest on the assumption of its correctly representing the effect of a single undulation on the organ operated upon ; and, admitting this to be the case, the true measure of intensity must be the square of the maximum velocity divided by the periodic time ; so that instead of the measure of intensity being some constant multiple of the ratio of the squares of the amplitude and periodic time, *it must be a constant multiple of the ratio of the square of the amplitude to the cube of the periodic time.*

My reason for considering that the *square* of the amplitude cannot enter as a factor into the expression for the intensity is very simple.

If we have two series of waves superposed, each of which is represented by

$$y = a \sin \frac{2\pi}{\lambda} (vt - x),$$

the resultant vibration will be represented by

$$y = 2 a \sin \frac{2\pi}{\lambda} (vt - x) ;$$

from which it follows, if the square of the amplitude enters into the expression for the intensity, that the two systems of vibrations combined will produce *four* times the amount of illumination (supposing light to be referred to) which either would produce separately—a conclusion which appears absolutely fatal to this mode of estimating the intensity*.

That the data upon which Mr. Bosanquet founds his experimental determination of the measure of intensity are precarious, must, I conceive, strike every one. That the result he obtains is inadmissible, appears to follow from the following considerations.

Let a, a_1 be the amplitudes of two notes at opposite extremities of the musical scale ; separated, say, by *seven* octaves. Then, if τ be the periodic time of the one, $2^7. \tau$ will be that of

hension, inasmuch as the particles of air in contact with the tympanal membrane must necessarily have the same velocity as the latter.

If I remember rightly, Fresnel distinguishes between the intensity of the vibration itself and that of the sensation resulting from it—expressing the former by the simple power, the latter by the square of the maximum velocity.

* The only attempt to prove that the square of the amplitude and not its simple power should be taken, with which I am acquainted, assumes as a postulate that two candles will give *twice* the illumination of one ! See Airy's ' Tract on the Undulatory Theory ' *in loco*.

the other.   Suppose, now, that the intensities of the two notes
are equal; then, according to Mr. Bosanquet's measure of
intensity, we shall have

$$\frac{a^2}{\tau^4} = \frac{a_1^2}{2^{28} \cdot \tau^4},$$
$$\therefore \ a_1^2 = 2^{28} \cdot a^2$$

and

$$a_1 = 2^{14} \cdot a \ ;$$

i. e. the amplitude of excursion of the lower note will be upwards
of *sixteen thousand times* as great as that of the higher—a con-
clusion which appears incredible.

   In reference to the principle on which the definition of
intensity must be determined, I may observe that we have no
direct consciousness of the amount of a velocity or force
impressed alike upon all parts of our frame; it is only when
the velocity or force produces relative displacement of different
parts of the system that we become conscious of its existence,
and are enabled to measure its effects.   The amount of dis-
placement of the nerves of the eye or ear is therefore neces-
sarily one element in the expression for the intensity of the
ray or note.   The only other element remaining to be taken
into account is the time within which such displacement is
effected.   Till the contrary is shown by indisputable experi-
mental evidence, I must contend that the simple ratio of one
of these elements to the other is the only measure of the inten-
sity of a light or sound which can be regarded as admissible.

   6 New Square, Lincoln's Inn,
        December 6, 1872.

V. *On the Magnetizing-Function of Soft Iron, especially with
   weaker decomposing-powers.   By* Dr. A. Stoletow, *of the
   University of Moscow* *.

[With a Plate.]

IN Kirchhoff's generalization† of Poisson's theory of the mag-
   netization of soft iron, the knowledge of a certain empiric
function is of the greatest importance.   This we will name the
magnetizing-function of iron, and denote it by k.

   In order to render palpable to ourselves the physical signifi-
cation of this quantity, we have to imagine an infinitely long and
thin iron cylinder in a homogeneous magnetic field; the mag-

   * Translated from a separate copy, communicated by the Author, from
Poggendorff's *Annalen*, vol. cxlvi. pp. 439–463, having been laid before the
Moscow Mathematical Society on Nov. 20 (December 2), 1871.
   † Crelle's *Journal*, vol. xlviii. p. 370.

netic force is directed along the axis of the cylinder, and its magnitude is R. The iron is then magnetized uniformly throughout its whole length; that is, the magnetic moment m, referred to the unit of volume, is the same in every point of the cylinder. The ratio $\frac{m}{R}$ we designate as the value of the magnetizing-function for the argument R. If M, L, and T represent the units of mass, length, and time, R as a *magnetic force* is a quantity of the dimensions $M^{\frac{1}{2}} L^{-\frac{1}{2}} T^{-1}$; the quantity $k(R)$ is a *pure number*.

If, now, k is known for every value of R, we have all that is necessary to enable us to determine theoretically the magnetization of any mass we please of isotropic iron of which the form and dimensions are known and which is in a given magnetic field, so far as the coercive force of the iron can be neglected. It is true that only a few special cases can be considered from this point of view; but this is, perhaps, not owing to the indeterminateness of the question, but solely to the analytical difficulties of the solution.

On the course of k when R becomes greater or less, as well as on the question how far it turns out different for different sorts of iron, the information at present existing is still rather unsatisfactory. Most observers have experimented with cylindrical rods—a case in which a strict theory can only be carried out on the assumption that the rod is of infinite length and thinness. On the other hand, the magnetizing-force made use of, and the magnetic moment it produces in the iron, have for the most part not been given in *absolute measure*, which makes the calculation of k impossible. So far as I know, such absolute measurements have only been made by Weber and Von Quintus Icilius.

The latter had to do with iron ellipsoids (in a homogeneous magnetic field); while Weber used cylindrical rods, which could only be considered by way of approximation, as very extended ellipsoids, and to that extent admit a theoretical treatment.

Neither of the two physicists mentioned has calculated the values of k from his experiments; they contented themselves with the consideration of the magnetic moment of the mass of iron. This, however, is not adapted to show clearly the universal dependence of the magnetization upon the magnetizing-force, since the magnetic moment of a cylinder of unlimited length, or an ellipsoid, is conditioned not only by this force, but also by the *form* of the iron.

Kirchhoff* first, from Weber's measurements†, calculated the

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* Crelle's *Journal*, vol. xlviii. p. 374.
† *Electrodyn. Maassbest.* iii. art. 26.

values of the function k for certain values of its argument, in which for the cylindrical form of the iron an ellipsoidal one approximating to it as nearly as possible was substituted. The following numbers resulted :—

R.	k.	R.	k.
296	25·0	1512	8·4
301	23·5	1583	8·1
612	16·9	1773	7·4
823	13·5	1975	6·7
967	12·0	2080	6·4
1184	10·2	2397	5·7
1297	9·5	2484	5·6

As unit for R, $\dfrac{\text{mgr.}^{\frac{1}{2}}}{\text{mm.}^{\frac{1}{2}} . \text{sec.}}$ was taken, after Gauss.

From this we see that, with rising values of the argument, the function k diminishes, at first rapidly, then more slowly, and approaches asymptotically either zero or infinity—a fact already indicated previously by the observations of Joule and Müller.

The same method of calculation can with greater right be applied to the more recent experiments of Von Quintus Icilius [*], because the form of the iron was really ellipsoidal. Some of these experiments were also performed with feebler magnetizing forces, as, instead of the direct magnetic action of the iron, the induction currents excited in a spiral wrapped round the ellipsoid, on the reversal of the magnetizing current, were measured.

For the calculation of k from these experiments, however, we must use only the most elongated ellipsoids, because with others the influence of k on the quantity of the magnetic moment is inconsiderable, and almost vanishes in comparison with the influence of the *form* of the ellipsoid. We will therefore calculate the experiments with both ellipsoids ($l=199$, $d=1\cdot97$, and $l=350$, $d=2\cdot12$; l is the polar axis, d the equatorial axis, both expressed in millimetres). If m is the magnetic moment of an extended ellipsoid of rotation which is magnetized by a constant force X acting parallel to the polar axis, we have

$$m = k\text{R} = \frac{k\text{X}}{1 + k\text{S}},$$

where S represents a number to be calculated from the ratio of the axes of the ellipsoid, viz.

$$\text{S} = 4\pi\sigma(\sigma^2 - 1)\left(\frac{1}{2}\log\text{nat}\frac{\sigma + 1}{\sigma - 1} - \frac{1}{\sigma}\right),$$

[*] Pogg. *Ann.* vol. cxxi. pp. 134 & 137.

if $\sigma = \dfrac{1}{\sqrt{l^2 - d^2}}$ *. The application of these formulæ to the two
ellipsoids above mentioned gives the following Tables :—

TABLE I.

R.	k.	R.	k.	R.	k.
2·40	30·5	33·1	119·0	53·3	110·9
5·20	40·8	33·9	118·7	59·2	113·0
12·0	72·5	38·6	120·2	98·4	89·3
21·1	99·1	45·6	120·4	176·2	62·9
24·1	113·4	51·9	119·1	300·7	39·7

TABLE II.

R.	k.	R.	k.	R.	k.
5·18	20·1	116·5	76·8	1722	7·11
8·71	22·6	148	64·9	2034	6·06
10·30	23·1	213	47·1	2044	6·05
14·30	28·4	240	41·9	2449	5·37
22·2	45·3	250	40·7	2981	4·28
26·9	54·3	379	27·9	3013	4·23
34·4	83·4	455	23·8	3464	3·73
38·5	94·5	495	21·9	3864	3·36
47·0	98·1	610	18·1	3971	3·25
49·2	107·5	749	14·9	4229	3·05
64·9	107·3	935	12·3	4541	2·86
97·2	87·0	1339	8·88		

Hence becomes evident the remarkable fact which, it seems
to me, has not yet been duly recognized, that with the lower
values of R the magnetizing-function has an ascending course,
and with a certain value of R reaches a maximum. We
see further from these Tables that with a very long and thin
rod, if it is magnetized by a force not too great, the magnetic
moment increases not as is usually assumed, nearly proportion-
ally, but much more rapidly, and between certain limits of the
force is nearly proportional to the cube of it.

This is to be seen even from some of Joule's experiments†,
whose attention, however, was chiefly directed to the *permanent*
magnetism of the rod. Further, Wiedemann ‡ remarked that,
with rods of moderate thickness, the magnetic moment increases
a little more quickly than the magnetizing-force. From the series
of experiments No. 1 of Von Quintus Icilius the fact comes out
very strikingly and at once ; prominence was given to it by the
observer himself : he seems, however, to be surprised that the

* Neumann, Crelle's *Journal*, vol. xxxvii. p. 44.
† Phil. Trans. 1856, p. 287.   ‡ *Galvanismus*, vol. ii. p. 297.

augmentation of $\frac{m}{X}$ does not occur in the same degree with all ellipsoids, and thinks that "it may be too soon, in the present state of our knowledge, to expect to deduce a determinate law in relation to this"[*].   And yet this dissimilar behaviour of different ellipsoids is a direct consequence of the theory.

Indeed, let us first contemplate the two less-elongated ellipsoids numbered 2 and 3 in v. Quintus Icilius, and which were cut out of the same piece of iron as No. 1 ($l=199$, $d=1·97$). For No. 2, l was $=200$, $d=20·41$; for No. 3, $l=51$, $d=19·84$. We may therefore, in the expression

$$\frac{m}{X} = \frac{1}{\frac{1}{k}+S},$$

neglect $\frac{1}{k}$ in comparison with S, or put $\frac{m}{X} = \frac{1}{S}$.   We then get $\frac{m}{X}=3·80$ for No. 2, and $0·608$ for No. 3.

From the experiments of v. Quintus Icilius there result the means   4·34 for No. 2, and 0·596 for No. 3.

M. v. Quintus Icilius[†] has, further, investigated also a more elongated ellipsoid ($l=100·5$, $d=5·24$), with which the approximative calculation just used would be inadmissible.   In order to test the theory in this case also, let us proceed as follows. First, for every X given we seek the corresponding R, using Table II. (as the more extended): that is, we first put $k=0$, therefore $R=X$; we find for this value of R the corresponding k from the Table, calculate again $R=\frac{X}{1+kS}$, &c. until two consecutive values of R come out nearly equal.   Then we have found k, and can calculate $\frac{m}{X} = \frac{1}{\frac{1}{k}+S}$ and compare it with the result of experiment.

In this way I find, for example,

X.	$\frac{m}{X}$ calculated.	$\frac{m}{X}$ observed.
48·2	7·6	7·09
275	10·0	9·67
553	10·0	9·99
1701	6·5	7·51
2851	4·5	5·00
4436	2·9	3·52

* Pogg. *Ann.* vol. cxxi. p. 155.   † *Ibid.* pp. 132 & 138.

We see that here also observation and calculation do not diverge too widely.

Before I leave the experiments with ellipsoids and pass to my own investigation, I must mention a still more recent work (1870), that of M. Riecke*. He has observed, according to Weber's method, the remagnetizing of various ellipsoids by the vertical component of terrestrial magnetism. The more lengthened the ellipsoid, the greater was, in general, the number resulting for k. In conformity with the foregoing, this was to be expected—since, X remaining equal, the quantity R increases simultaneously with $\frac{1}{S}$,—although M. Riecke is more inclined to seek another reason for it. The magnetizing-force was not measured directly; but if we assume, with Weber, that the vertical component of the earth's magnetism (at Göttingen) was $=4\cdot228\dagger$, then it follows that, for Riecke's experiments, $R=0\cdot31-0\cdot72$. Corresponding to this, k increased from $13\cdot5$ to $25\cdot4$.

It seemed to me not without interest to ascertain the magnetizing-function by another method, recently proposed by Kirchhoff‡. Therein I experimented especially with feebler decomposing forces, in order once more to establish and place beyond doubt the ascending course of k with such forces. The experiments which I will communicate appear also to have a further interest. The only case of magnetizing theoretically solved completely, and which at the same time can be carried out in practice, was till quite recently that of an ellipsoid (inclusive of the sphere). In the present experiments, I believe, the theory is for the first time tested on a body of another form, namely a *ring*.

We imagine a ring of iron—that is, a solid of rotation which is not touched by the rotation-axis. Let this ring, in its whole periphery, be wrapped round with wire (the *primary wire*); and let another wire (the *secondary wire*) be wound round it once or more times. If a constant current is passed through the first wire, and if the second is closed upon itself, a momentary current is induced in the latter as soon as the direction of the primary current is suddenly reversed§. The integral value of the

* *Die Magnetisirungszahl des Eisens für schwache magnetisirende Kräfte*: Göttingen, 1871. Abstract in Pogg. *Ann.* vol. cxli. p. 453.
† This number belongs properly to the *middle* of the year 1870. See Weber, "Bestimmung der erdmagnetischen Kraft in Göttingen," p. 30 (*Abhandlungen d. k. Gesellschaft d. Wiss. zu Göttingen*, vol. vi.).
‡ Pogg. *Ann.* Ergzbd. v. p. 1.
§ I always employed the *reversal of the current*, because thereby the results are less vitiated by residual magnetism than with the *closing and opening* of the circuit. The same method was made use of by Weber and v. Qu. Icilius. The employment of *both* methods would enable us to measure the residual magnetism of the iron.

induced electromotive force, expressed in absolute electromagnetic measure, is, according to the theory given by Kirchhoff:—

$$E = 4nn'i \{ 4\pi kM + P \}. \quad . \quad . \quad . \quad . \quad (1)$$

The first term of the expression proceeds from the currents induced by the remagnetizing of the iron; the second, from the direct voltaic induction of the two wires.   Herein signify:—

n and n', the numbers of the windings of the primary and secondary wires respectively (if the latter is wound round the ring ν times in one direction, ν' times in the other, we have to understand by n' the difference $\nu - \nu'$);

i, the intensity of the primary current, in absolute electromagnetic measure;

M, the integral, extended to the cross section of the ring, of the form $\int \frac{dS}{\rho}$, in which dS is an element of the surface of that section, ρ the distance of this element from the rotation-axis of the ring;

P, a similar integral, referred to the surface of a primary winding.

k is the magnetizing-function of the iron; and the argument R, to which k is referred, is the mean value of the magnetizing-force. This is $= \frac{2ni}{\rho}$ for a point (ρ) of the ring.   Consequently

$$R = \frac{2niM}{S}, \quad . \quad . \quad . \quad . \quad . \quad (2)$$

S denoting the entire surface of the cross section of the iron.

If, then, we know the form and dimensions of the ring and the primary turns, as well as the number of these and the secondary rounds, the function k can be calculated for every given R, to be expressed in absolute measure, as soon as the ratio $\frac{E}{i}$ is likewise measured in absolute measure.

This is the fundamental idea of the method recommended by Kirchhoff and which I have followed. To what extent it required to be modified for different values of the magnetizing-force R, will be gathered from the following.

I had such a ring made of soft iron, in the workshop of Dr. Meyerstein, at Göttingen. It was kept for twelve hours at a red heat, and then cooled by gradually covering the fire. The cross section of the ring is a rectangle; the extreme diameter 1 found equal to 200·025 millims., the internal = 180·37 millims.; the height = 14·75 millims. From this the quantity denoted above by M is calculated = 1·526 millim.

To this ring two rings of wood, circularly rounded off on the

outer side, were cemented; upon these a covered copper wire
(without its covering, 0·45 millim. thick; with it, 0·67) was
wound as close and uniformly as possible. This wire, mostly
used as primary closing, had 800 turns; it was connected with
a galvanic series. The mean contour of a winding is pretty
accurately represented by the combination of a rectangle 11·1
millims. wide, and 24·5 high, with two semicircles of 11·1 dia-
meter (Pl. III. fig. 1). Accordingly the quantity denoted by P
(upon the exact knowledge of which much less depends than
upon the determination of M) is found by calculation to be 3·87
millims.

Equations (1) and (2), applied to the ring described, give us
the following formulæ for the calculation of R and k :—

$$k = \frac{\dfrac{1}{320\,n'} \cdot \dfrac{E}{i} - 3·87}{19·172}; \quad R = 16·84\,i.$$

Upon the first layer of wire-turns, which mostly served alone
for the primary wire, 750 rounds of the same piece of wire were
wound, and likewise filled up the entire periphery of the ring;
in this second layer, however, the wire was divided into five
separate portions, of which the number of circumvolutions were
respectively 50, 100, 150, 200, and 250. As required, one or
another of these divisions, or different combinations of them
were connected with a multiplier and used as a secondary wire.
The number of the secondary circumvolutions might thus be in-
creased, step by step, to 100, 150, ... 700, 750; and, with the
same *effective* number of turns, I could vary the resistance of the
secondary wire, by using, for example, at one time the 50 divi-
sion alone, at another combining the 250 and 200 divisions in
opposite directions. All these wires were wound in such wise
that with each the *longitudinal* current, according to Ampère,
was compensated by a returning round of wire. (Fig. 2 is a
sketch of the inner wire.)

With greater decomposing forces I could make use of a smaller
number of secondary turns. Then I had no need of the divi-
sions above mentioned for the secondary closing, and merely
caused the conducting wire of the multiplier to run round the
ring in ten turns. Those divisions, however, I could now con-
nect with each other, all in one direction, and with the first
layer of wire; whereby the number of the primary circumvolu-
tions became 1550, and with the same galvanic series the de-
composing force was considerably greater. With this arrange-
ment, taking into account the alteration of n and P (with the
outer turns the length of the oval was 37·75 millims., the breadth

11·75), the following equations became valid :—

$$k=\frac{\dfrac{1}{6200n'}\cdot\dfrac{E}{i}-4\cdot11}{19\cdot172}; \quad R=32\cdot626\,i.$$

Before passing to the proper measurements, I will give an account of two preliminary experiments, which I made for the purpose of ascertaining the regular course of the phenomenon and to test its accordance with theory.

If we leave the magnetizing current i unchanged, from equation (1) it follows :—1, that the induced electromotive force increases proportionally with the number of the secondary turns ; 2, that it depends *only* on the number, and not on the quality, of the turns.

Lenz found similar laws by experiment when investigating the induced currents excited in a spiral wire enclosing an iron cylinder as soon as the cylinder was pulled away from the pole of a powerful steel magnet*. Theoretically, however, these laws can only be derived under the assumption that the cylinder is infinitely thin and long†.

I conducted the current of a Daniell's series through the primary wire of the ring. Divisions 100, 150, 200, and 250 of the secondary wire were connected with each other and with a galvanometer of great deadening force. The connexion was made in various ways, so that the numbers of effective turns were respectively 100, 200, 300, 400, 500, and 700. As the resistance of the secondary circuit remained unaltered, the deflections observed on the galvanometer when the primary current was reversed were proportional to the induced electromotive forces. The mean numbers of these deflections, each from eight observations, were :—

with 100 turns,   200,   300,   400,   500,   700,
  = 47·2 scale-divisions; 94·4; 140·4; 189; 236·4; 329·9.

Starting from the number 329·9, the others are found by calculation to be

47·13,   94·26,   141·4,   188·5,   235·6.

When the divisions were so combined that the number of turns running in one direction was equal to the number in the opposite direction, I obtained on the reversal of the current the small deflections ±1·5, which are probably to be attributed to the imperfect homogeneousness of the iron mass or of the primary turns‡. When 150−100−50 turns were employed, the magnet remained perfectly at rest.

* Wiedemann, *Galvanismus*, vol. ii. p. 634.
† Kirchhoff, Crelle's *Journal*, vol. xlviii. p. 368.
‡ According to theory, the action of the ring upon external magnets

Similarly it was proved that the form and other conditions of the secondary turns are without influence. When I caused a thicker wire to run, in 50 wide and irregular turns, round the ring, and formed a secondary closing out of this wire, the oppositely directed division of 50 turns, and the multiplier, I obtained a sensible deflection on reversing the primary current.

Generally the consecutive induction-impulses are very constant, if only the precaution is taken to turn the commutator several times after each change of the magnetizing-force before commencing the observations, as the first deflections are influenced by residual magnetism.

I pass now to the proper measurements of k and R.

As we have seen, it depends upon ascertaining the ratio $\frac{E}{i}$ of the induced electromotive force to the intensity of the inducing current. For this purpose, according to the strength of the latter, various methods were employed.

With stronger primary currents, the arrangement shown in fig. 3 (Pl. III.) was adopted. The current of a galvanic apparatus K (mostly 4–12 Daniell's elements, for more powerful currents 12–14 Bunsen) was conducted through two commutators (C_1 and C_2), the primary wire P of the ring, and a circular roll of wire (R) of known dimensions. This roll, intended for the measurement of the primary current, is placed perpendicular to the magnetic meridian, eastward of a magnetometer (M); the axis produced of the roll meets the middle point of the magnetic bar. The distance of the latter from the centre of the roll was mostly fixed at 1000 or 1250 millims.

Moreover the magnet M is now surrounded by a multiplier with close windings. Through this multiplier, provided with a damper, the induced currents are conducted. That is to say, the conducting wire of the multiplier is either wound in several turns round the ring, or connected with the above-mentioned divisions of the second layer of wire upon the ring. Lastly, W is a Siemens's resistance-scale, by which the primary current can be weakened. The observation was effected with scale and telescope.

The constant current in the roll R imparts to the magnetometer a certain deviation from the magnetic meridian. By turning the commutator C_1, the current in the primary wire P only is reversed. Thereby an induction-shock is produced in the mul-

should have been altogether independent of whether a current passed through the turns and in which direction. This, however, was far from being verified when I placed the ring close beside the galvanometer—which again intimated the before-mentioned want of homogeneousness. At all events the direct action of the ring upon the galvanometer was quite imperceptible when it held its usual place.

tiplier, and imparts a deflection to the magnet. The position of
equilibrium conditioned by the primary current is observed,
as well as the induction-deflection. By a second turning of C_1
the magnet, after coming to rest, receives an impulse in the
opposite direction. Finally, the same observations are made
after the commutator C_2 has been turned and thereby the cur-
rent in the entire primary circuit reversed.   Let

  a be half the difference between the two positions of equili-
    brium of the magnet with the two positions of the commu-
    tator C_2,
  A the elongation of the magnet from the then position of
    equilibrium by the induction-shock,
  T the time of an oscillation of the undamped magnetometer,
  λ the logarithmic decrement of the oscillations when the se-
    condary wire is closed,
  m the ratio of the torsion-moment exerted by the multiplier
    upon the magnet to that proceeding from the roll, the same
    current passing through both ;

then the ratio of the induction-shock **J** to the primary current
i, both taken in the same measure, is

$$\frac{J}{i} = \frac{A}{ma} \cdot \frac{T}{\pi} \cdot e^{\frac{\lambda}{\pi\mu}\arctan\frac{\pi\mu}{\lambda}},$$

where μ is the modulus of the Briggian logarithms.

If, instead of observing the first deviations, we wish to employ
the multiplication method, A and *a* are to be calculated, in the
known manner, from the consecutive readings and the damping λ.

Therefore, *m* and T being known (T varies a little, and must
be determined afresh from time to time), the ratio $\frac{J}{i}$ can be cal-
culated from determinations each time of A, *a*, λ.   If, further,
the resistance W of the secondary closing is known in absolute
measure, we have also the ratio $\frac{E}{i} = \frac{J \cdot W}{i}$, which is required for
the calculation of *k*.

The resistances of all the wires of which, in the various ob-
servations, the secondary circuit consisted—that is, of the mul-
tiplier M (w_m) and all the sections of the second layer of wire
($w_{50}, w_{100} \ldots$)—were determined in absolute measure by compa-
rison according to Wheatstone's method with a British Asso-
ciation unit.   They were (reduced to $20°\cdot4$ C.) :—

$$w_{m} = 2\cdot4758 \times 10^{10}\frac{\text{mm.}}{\text{sec.}} \qquad w_{150} = 2\cdot1150 \times 10^{10}\frac{\text{mm.}}{\text{sec.}}$$
$$w_{50} = 0\cdot74502 \quad ,, \quad ,, \qquad w_{200} = 2\cdot6829 \quad ,, \quad ,,$$
$$w_{100} = 1\cdot4046 \quad ,, \quad ,, \qquad w_{250} = 3\cdot4253 \quad ,, \quad ,,$$

In every observation, the temperature of the air close to the multiplier was noted ; and a second thermometer was applied to the second layer of the ring, which with more powerful primary currents was considerably heated by contact with the primary wire. The resistances were reduced to the readings of the two thermometers ; and herein the increase of a resistance w (of copper) with a heating of 1°C. could be supposed equal to $0 \cdot 00387\, w$*.

The number above denoted by m, which specifies in what ratio the action of the multiplier was more powerful than that of the roll (the currents in both being equal), was measured once for all. For this purpose the current from a galvanic apparatus was conducted through the roll ; a known portion of the same current was sent through the multiplier by means of a derivation of small, accurately measured resistance. There was found $m =$ 2414 for the case in which the roll was 1000 millims. distant from the suspending thread of the magnet. From the constants of the roll to be given below, the corresponding number m could be calculated for the case in which the distance was different.

We have now all that is necessary for the calculation of k from the observed A, α, λ, and T. In order, however, that we may be able to calculate also the argument R (the magnetizing-force) to which this k refers, the intensity of the current i must be determined in absolute measure. Let

    H be the horizontal direction-force of the magnet (proceeding from the magnetic field of the place of observation, in a small part also from the torsion of the suspension-thread),

    u the angle of deflection of the magnet, produced by a current of absolute value i which is passing through the roll,

    F the superficial extent of the roll,

    r the distance of the roll from the thread of the magnet ;

then we have, as a first approximation :—

$$\frac{\mathrm{H}\tan u}{i} = \frac{2\mathrm{F}}{r^3}.$$

More exactly, the right-hand part of this equation is a series which proceeds by descending powers of r. Presupposing that both magnet and roll, in their distant magnetic action, are symmetrical in relation to their axes, only odd powers of r can occur in that series. Limiting ourselves to the first two terms, we obtain

$$\frac{\mathrm{H}\tan u}{i} = \frac{2\mathrm{F}}{r^3}\left(1 + \frac{\beta}{r^2}\right),$$

where β is a constant † which depends on the dimensions of the

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* Matthiessen and v. Bose ; see Wiedemann, *Galvanismus*, vol. ii. p. 1060.

† The influence of the angle u on β may be neglected when u is small.

E 2

roll and the magnet, as well as on the distribution of the magnetism in the latter.

From the dimensions of the roll and the numbers of the convolutions of its four layers of wire, there was found

$$F = 6075500 \text{ sq. millims.}$$

The second constant β could now be ascertained by experiment. To that end the angles of deflection u_1 and u_2 of the magnet were observed, which were occasioned by the roll when, traversed by a constant current, it was placed at two different distances r_1 and r_2. Then evidently

$$\beta = - \frac{\dfrac{\tan u_1}{\tan u_2} - \left(\dfrac{r_2}{r_1}\right)^3}{\dfrac{\tan u_1}{\tan u_2} - \left(\dfrac{r_2}{r_1}\right)^5} \cdot r_2^2.$$

In this determination, the most advantageous ratio $\dfrac{r_2}{r_1}$ is found by the rules of the calculation of probabilities to be $= 1{\cdot}336$. Accordingly with $r_1 = 1000$ millims. r_2 was taken as $= 1335$ millims. In this way I obtained $\beta = -26301$, and therefore

$$\frac{H \tan u}{i} = 0{\cdot}011831 \quad \text{for } r = 1000 \text{ millims.,}$$
$$= 0{\cdot}0061169 \text{ for } r = 1250 \quad ,,$$

The absolute intensity of the current can be calculated according to these formulæ, provided that the quantity H is known. This was measured thrice in the course of the investigation, by Gauss's method. In order to take account of the variations of H in the intervals, the deflection-bar used in the measurement of H was daily placed upon the board which carried the roll, at the distance of 1250 millims. from the latter, and the deflection of the suspended magnet was observed; from this the alteration of H since the last measurement could be ascertained. It is true that the magnetic moment of the bar was not altogether constant; the variation, however, was caused chiefly, perhaps, by the temperature, and could still be allowed for. I will now call attention to some sources of error in the method described. If we so form the secondary closing that the number of effective turns is $= 0$, small deflections of the magnet are to be expected, even independently of the unhomogeneousness of the iron and of the primary convolutions, as soon as the position of the commutator C_1 is changed. These arise partly from the interruption of the primary current, but partly from the extra currents which are induced in the primary wire of the ring as well as in the roll. The first and third of these disturbances act, as may

easily be seen, in opposition to the primary current; while the action of the extra currents induced in P changes its direction both with the initial placing of C_1 and with the placing of C_2. Hence arise small movements of the magnet, the direction of which changes with the placing of C_2, but their magnitude with the nature of the change of position of C_1. This was actually observed. It is not difficult to estimate the influence of this source of error both upon the calculation of A and on the values of a (when these are determined from multiplication-readings), and to apply the correction. Moreover the correction of A can be dispensed with, if the secondary wire be inserted in the multiplier now in one direction and then in the opposite.

As an example of the method here discussed, I cite, abridged, the record of one measurement:—

19*th October* 1871; Series, 12 Daniell's elements weakened by 10 Siemens's resistance. Roll at 1000 millims.

Number of primary turns $n' = 250 - 200 + 150 - 100 = 100$.

Temperature at the ring, $t_r = 16°·7$ C.; temperature at the multiplier, $t_m = 9°·7$. Hence $W = 11·874 \times 10^{10} \frac{\text{millims.}}{\text{second}}$.

Distance of the scale from the mirror of the magnet $= 2125·3$ scale-divisions.

Logarithmic decrement $\lambda = 0·1410$; consequently

$$\log e^{\frac{\lambda}{\pi \mu} \arctan \frac{\pi \mu}{\lambda}} = 0·06588.$$

Duration of an oscillation of the magnetometer undamped, $T = 20·419$ seconds.

Direction-force of the same, $H = 1·9820$ milligr.$^{\frac{1}{2}}$ millim.$^{-\frac{1}{2}}$ sec.$^{-1}$.

The eight positions of equilibrium of the magnet (with three combinations of C_1 and C_2 and both ways of connexion of the secondary wire with the multiplier) were, corrected for the final distance of the scale from the mirror:—

621·5	404·8
622·2	406·2
623·3	405·9
621·7	405·4
Means . . 622·17	405·57

The corresponding movements of the magnet through induction-shocks were:—

243·4	244·1
241·7	244·5
241·9	244·4
243·7	244·4

Consequently $a = 108\cdot23$ sc.; $A = 243\cdot46$ sc.
Hence we obtain

$$\frac{E}{i} = 8\cdot3700 \cdot 10^8 \frac{\text{millims.}}{\text{second}}, \quad i = 4\cdot2656 \frac{\text{mm.}^{\frac{1}{2}}\,\text{mgr.}^{\frac{1}{2}}}{\text{sec.}},$$

and, lastly,

$$k = 136\cdot22; \qquad R = 71\cdot830 \frac{\text{mgr.}^{\frac{1}{2}}}{\text{mm.}^{\frac{1}{2}}\,\text{sec.}}.$$

With feebler currents this method required a modification, as the measurement of such currents by removing the roll would be too inexact. This could be remedied by inserting a bridge before the wire P, so that only the current in P would be weakened. Mostly, however, for such cases the arrangement was adopted which Kirchhoff employed in his measurement of the induction-constant. The series K (Pl. III. fig. 4), the primary wire P of the ring, the secondary wire S, and the multiplier M of the magnetometer formed a circuit, which was divided into two branches by means of a bridge B of small resistance. By the commutator C_1 the current in P was reversed, and by the commutator C_2 the disposition of the wire of the multiplier was changed.

The currents induced in S when C_1 is shifted pass almost exclusively in the circuit SBM; while the current from the galvanic apparatus passes for the most part in K B P, only a small portion of it going through M and being used for the measurement. As the reduction-constant of the multiplier, which serves for the determination of absolute current-intensities from observed readings, had been previously ascertained, the current i_m in the multiplier could be calculated in absolute measure. Finally, from the known resistances of M, S, and B the whole magnetizing current i in P was obtained.

Now, with respect to the quantity $\frac{E}{i}$ required for the calculation of k, it is easily seen that it becomes $= w_b \frac{J_m}{i_m}$, if w_b denotes the absolute resistance of the bridge B, and J_m the integral value of the induction-current in the multiplier. (It is here presupposed that w_b^2 may be neglected in comparison with the product of the two resistances K B P and S B M, which was admissible in all the experiments.)

As a bridge B strong copper wires were used, each with its ends soldered to two small forks of thicker wire, which were amalgamated beneath and dipped into mercury-cups (fig. 5). The resistance of the proper wire 1B2 (from the place of soldering, 1, to the other, 2) is to be regarded as the quantity denoted

by w_b. This resistance was determined by Thomson's method[*], with only a slight modification, which was required by the circumstance that I had not to measure off two resistances according to a given ratio, but to compare each two already definitively measured and prepared resistances. In this manner, for the four bridges used, the resistances w_b at $20^\circ\!\cdot\!4$ C. were found to be respectively equal to

$$1\cdot1626 \times 10^8 \tfrac{\text{mm.}}{\text{sec.}}, \quad 2\cdot2589 \times 10^8 \tfrac{\text{mm.}}{\text{sec.}},$$
$$1\cdot1683 \times \text{ ,, \quad ,, } \quad 2\cdot2811 \times \text{ ,, \quad ,, ;}$$

so that by using the two small wires in juxtaposition a resistance is obtained which amounts to less than $0\cdot002$ of the least value of S M B.

In all other respects the method of measuring was identical with the one first described : now the first deflections of the magnet were observed ; then the multiplication method was employed. The disturbing influence of the extra currents (which now proceeded only from the wire P), as well as of the interruption of the current, tends always to diminish the deflection of the magnet, which is occasioned by the primary current and has to be calculated from multiplied readings. This influence it is easy to eliminate ; the mean value, however, of the induction-deflections is not affected by it, if we always carry out the two shiftings of the commutator C_1.

I give also an example of this second method :—

September 3, 1871. One Daniell ; $n = 800$; secondary wire = division of 100 turns. Consequently $\dfrac{w_s + w_b}{w_b} = 668\cdot30$ (w_s is the resistance of the branch B M S B, fig. 4).

In B the two smaller bridges were inserted in juxtaposition ; temperature $= 21^\circ\!\cdot\!2$; consequently $w_b = 5\cdot8687 \times 10^7 \tfrac{\text{mm.}}{\text{sec.}}$.

Further

$$T = 20\cdot640, \quad \lambda = 0\cdot16571, \quad H = 2\cdot0018, \quad D = 2261 \text{ sc.}$$

The multiplications (10 readings each) gave as positions of equilibrium of the magnet,

$$409\cdot80, \quad 584\cdot67, \quad 584\cdot78, \quad 409\cdot68,—$$

and as the values of the induction-deflection,

$$124\cdot23, \quad 122\cdot83, \quad 122\cdot64, \quad 123\cdot90.$$

Correction for extra currents $= 1\cdot84$.

From this we find

$$a = 89\cdot33 \text{ sc.}, \quad \Lambda = 123\cdot10 \text{ sc.},$$

[*] Phil. Mag. S. 4. vol. xxiv. p. 149 ; Wiedemann, *Galvanismus*, vol. ii. p. 1046.

and, finally,

$$k = 103.33, \quad R = 15.57.$$

The results of my measurements are collected in the following Table :—

R.	k.	R.	k.	R.	k.	R.	k.
4·302	21·51	16·47	113·5	83·26	120·0	195·7	61·93
5·497	23·78	23·21	157·0	91·40	112·2	205·9	59·22
7·017	26·44	32·12	174·2	100·35	108·1	217·0	56·47
9·220	40·95	35·62	172·3	105·03	104·2	228·0	53·92
10·53	51·10	38·14	170·7	111·18	97·12	235·8	52·88
11·51	59·76	40·38	168·9	119·6	93·97	252·2	49·68
12·60	68·70	52·47	161·6	132·6	87·70	272·7	47·29
13·67	76·53	67·89	141·7	140·1	82·08	288·2	44·04
14·94	84·53	71·83	136·2	156·0	75·43	296·1	43·65
15·60	104·48	75·55	132·1	179·3	66·87	307·3	42·13

From these data the continuous curved line in fig. 6 is drawn ; its abscissæ represent the values of R, and its ordinates the corresponding values of *k*. The two dotted lines are drawn after the above-calculated series of experiments of Von Quintus Icilius. We see that at the higher values of R the course of all three curves is very similar, while at the lower values the curve obtained by me has a much steeper ascent.

As is well known, the number *k* varies when the temperature of the iron undergoes considerable variations. This temperature, in the case of my ring, could not be stated exactly. The temperature at the place of observation sank considerably during the time the investigation was proceeding (September and October 1871) ; but this was partly compensated by my using gradually more and more powerful decomposing forces, so that the ring was more and more heated by the primary current (partly also, perhaps, by the remagnetizing itself) ; therefore most of the measurements refer to 15°–20° C. A considerable augmentation of *k* appeared only when once, towards the close of the investigation, I intentionally caused the current from 14 zinc-carbon elements to pass through the primary wire until it was heated from 10° to 40° ; the results of such experiments are not comprised in the Table.

With certain arrangements for the positive determination of the temperature of the iron, similar methods could be employed for the investigation of the influence of temperature on the magnetizing-function. It would moreover be desirable to extend such measurements to various sorts of iron, in which the course of *k* might exhibit considerable deviations. It would also be to the purpose to make use of thinner rings for the measurements

than the one above described, because they must furnish more accurate results, provided that the form of the ring be verified with equal exactness*. It has not yet been possible for me to complete my work in the directions mentioned.

The closer investigation of this subject might present many and various points of interest. On the one hand, a deeper insight would be thereby gained into the essence of that molecular process which we designate as the magnetization of a substance. From the facts here considered it seems to follow that the hypothesis of the existence in iron of convertible molecular magnets, in the form in which it is developed by Weber†, does not correspond to the course of the phenomenon with feebler decomposing forces. On the other hand, more exact knowledge of the function k might be of practical utility, especially in the construction both of electromagnetic motors and of those magneto-electrical machines of a newer kind (those of Wilde, Siemens, Ladd, &c.) in which the temporary magnetizing of iron plays so important a part.

In conclusion, to Geh. Hofrath Kirchhoff, in whose laboratory this research was carried on, and who kindly assisted me with his advice, I most heartily express my gratitude.

Heidelberg, October 1871.

VI. *Experiments on Fluorescence.* By EDUARD HAGENBACH‡.

IT had long been observed that a certain number of solutions —for example, of nephritic wood (*lignum nephriticum*)— possessed the property of giving, in incident light, a peculiar reflection, quite different from the colour presented by the same bodies in transmitted light. Brewster and Herschel were the first to recognize that this property extended to a great number of substances ; they thought to explain it, the one by internal dispersion, the other by a peculiar reflection at the surface. It is to Stokes that we are indebted for having established that the change thus produced in the composition of the incident light does not result solely from reflection or absorption, but from the substance itself becoming luminous under the influence

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* Equation (1) for example, is strictly correct only under the assumption that in a cross section of the ring the number k can always be regarded as a *linear function* of ρ. The Table of results proves that even with my ring (in which the quantity ρ, or also the decomposing force, proportional to $\frac{1}{\rho}$, varies 10 per cent. within the cross section) this was almost everywhere admissible.

† *Electrodyn. Maassbest.* vol. iii. art. 26.

‡ Abstract, by the Author, of the complete Memoir in Poggendorff's *Annalen,* vol. cxlvi. pp. 65, 232, 375, & 508.

of incident light and emitting radiations whose refrangibility is different from that of the exciting rays. It was, again, this savant who introduced into science the word "fluorescence" to designate this phenomenon, on account of its being particularly noticeable on certain varieties of fluor spar.

Stokes studied a great number of substances with respect to their fluorescence, and in this way arrived at the important law that the refrangibility of the light of fluorescence is never greater than that of the exciting rays. The results obtained by Stokes have since been confirmed and extended by the labours of other physicists, particularly M. V. Pierre.

I was induced to undertake in my turn the study of this subject, because from different quarters there were sent to me, to be submitted to a more searching examination, substances possessing in a high degree this interesting property. I soon perceived that a theory of the singular phenomenon would not be possible until we possessed more exact observations than those which had hitherto been made. I extended my researches not only to these new substances, but also to those previously studied. I turned my attention more particularly to three points, namely :—

1st. Determination of the limits and the maxima of fluorescence : the question here was, to ascertain in what parts of the spectrum fluorescence begins and ends—and to see in each particular case if there was one or several maxima, always fixing their exact position.

The method which I made use of for this purpose consisted in the direct projection of the solar spectrum on the surface of the liquid.

2nd. Study of the absorption-spectrum of the fluorescent substances, and verification of the relation, first clearly established by Stokes, between absorption and fluorescence—consisting in this, that in all portions of the spectrum where there is fluorescence there is also absorption of the exciting rays.

3rd. Spectral analysis of the light emitted by fluorescence. Here the object was, to know the limits between which the spectrum produced by fluorescence extends, and to fix the position of the maximum or maxima, according as the spectrum presented only one or several luminous bands separated by dark portions. In this part of my work I applied myself to find out especially whether modifications in the composition of the incident light (that is to say, that which excites the fluorescence) involve modifications in the light emitted by the fluorescent substance : with this aim I employed different sources of light for its production.

The method employed in these researches was, to throw the

light of fluorescence, by a mirror properly arranged for the purpose, on the slit of a large spectroscope, with the aid of which it was analyzed.

I studied, in all, thirty-six substances. I shall mention here only some which are perhaps less known.

Solution of Morine Alum.—This liquid, which gives a fine fluorescence, is obtained (as M. Goppelsröder has shown) by dissolving precipitated morine alum (Cuba lac) in alcohol acidulated with chlorhydric acid.

Naphthaline-rose (called in England Magdala red) dissolved in alcohol.—This substance, one of the colouring-matters obtained from tar, was discovered by M. Schiendl, of Vienna, and was afterwards studied by M. A. W. Hofmann. It, too, exhibits a remarkable fluorescence, intense and beautiful, of yellow light.

Thiomelic Acid.—Whoever has made any elementary studies in chemistry knows that after heating for some time a mixture of alcohol and sulphuric acid, as is done in the preparation of olefiant gas, we obtain a thick greenish black residuum. M. Erdmann has given to this substance the name of thiomelic acid. M. Goppelsröder drew my attention to the remarkable fluorescence of this liquid.

Amide of Phthalic and *Amide of Terephthalic Acids.*—Under this double denomination I designate two substances, the solutions of which in alcohol and ether give a beautiful green and blue fluorescence, and which M. Hugo Müller, of London, has described as amides obtained by the reduction of nitrophthalic and nitroterephthalic acids.

Phthaleine of Resorcine or *Fluoresceine.*—This substance, which gives a magnificent green fluorescence, was sent me by M. Ad. Bæyer.

I shall not here give in detail the results of my observations; they will be found in full in my complete memoir in Poggendorff's *Annalen*; but I will come at once to the general conclusions set forth by my labours. And first I must remark that the property in question presents itself under the most varied forms. There can be no doubt that here we have to do with a phenomenon which, conformably to what takes place with most physical effects, depends essentially on molecular constitution and chemical composition—each substance appearing as an isolated individual with special characteristic properties, in such manner that it is very difficult to lay down general laws sufficiently exact. In the sequel I shall endeavour to demonstrate this view.

Fluorescence in the Spectrum or *Fluorescent Spectrum.*—To the question whether all the radiations of the spectrum are capable of exciting fluorescence, we can answer in the affirma-

tive. Only the rays beyond B can be excluded from the num-
ber of those which produce fluorescence; at least I know of no
substance which is fluorescent in that portion of the spectrum.
But when we consider that the light emitted by fluorescence is
less refrangible than the exciting rays, we can well understand
how it is that fluorescence excited in the extreme red is not
visible to our eyes.

As regards the greater or less extent of fluorescence in the
spectrum, great differences are found between different sub-
stances. There are cases in which fluorescence commences in
the violet, after the line G—for example, fluor spar, and a
solution which I first studied and which must contain bisulph-
anthrachinon; while in other cases it extends nearly through
the spectrum, as takes place with ethereal and alcoholic solution
of chlorophyl, alcoholic solution of naphthaline-rose and thio-
melic acid. In the direction of the extreme violet, fluorescence
always extends beyond the group H.

In respect of intensity in the different parts of the spectrum
this remarkable fact presents itself—that in a great number of
cases several different maxima of fluorescence are observed, sepa-
rated by bands of relative minimum. These maxima are not
all equally marked; the difference of brightness between them
and the minima likewise varies.

The number of these maxima also is very variable. Thus,
for example, seven are observed with fresh solution of chlorophyl,
five with solution of lampblack in alcohol or turpentine, three
with naphthaline-rose, alcoholic solution of turnsole, purpurine
in alum, and uranium glass, two with the alcoholic solution of
guaiacum resin, and only one with solution of morine alum and
thiomelic acid and with the solutions of sulphate of quinine,
æsculine, fraxine, photene or anthracene, petroleum, and nitrate
of uranium.

Correlation of Fluorescence and Absorption.—The fact that
wherever incident light produces fluorescence, absorption exists
at the same time, is a consequence of the law of the conserva-
tion of *vis viva* which it was easy to foresee. In my observa-
tions I continually saw absorption accompany fluorescence;
often the absorption-spectrum could even serve to determine
more precisely the maxima of fluorescence. What is still more
remarkable is the inverse fact recognized by Stokes, that, in
fluorescent substances in general, absorption is always accom-
panied by fluorescence, which was not necessarily to be fore-
seen; for in other coloured liquids there is often absorption of
light without fluorescence. On this last point in particular,
although my researches have fully confirmed the connexion
already known between the two, yet I have found cases in which

the substance studied exhibited in certain places a peculiar absorption in addition to that corresponding to the fluorescence. It is so, for example, with the aqueous solution of turnsol, solution of purpurine in soda, and nitrate of uranium either solid or dissolved.

Spectra of the Light emitted by Fluorescence, or *Fluorescence-Spectra*.—I have given particular attention to the study and description of fluorescence-spectra, which are presented under the most varied forms. I shall here notice only those which appear to me of special interest.

The different kinds of light already known to be emitted by fluorescence present the greatest variety in their colours, according as they are red, orange, yellow, green, blue, or violet; but it is only by spectral analysis that an exact idea can be obtained of their composition.

It is moreover established that there are very great differences in the breadth of the fluorescence-spectrum. That of chlorophyl has the least breadth; it is so narrow that we might almost suppose that the light thus emitted is a homogeneous red. Those of thiomelic acid, fluor spar, and others, on the contrary, exhibit a very great breadth; the fluorescence-spectra of these substances include rays belonging to the different portions of the spectrum from the red to the violet.

An interesting point to note is, that in a great number of cases the fluorescence-spectrum offers a very unequal distribution of luminous intensity: we have in them a series of brighter bands, maxima of luminous intensity, separated from one another by other bands more or less obscure; it is right, however, to remark that these bands are not sharply defined, but pass gradually into one another; and the dark bands are not absolutely black, but more or less shaded zones. I have observed, for example, eight maxima in the fluorescence-spectrum of nitrate of uranium, six maxima in that of photene or anthracene and in that of petroleum, five maxima with uranium glass and the extract of lampblack, three very distinct with solution of guaiacum, three less decided with fluoresceine, two equally well-marked maxima with alcoholic solution of turnsol, solution of orchil, and fresh solution of chlorophyl in ether, two maxima also, but less clear, with oxide of brasiline, sulphate of quinine, æsculine, and tincture of curcuma, and only one maximum (consequently no intermission in the intensity of the light) with solution of morine alum, naphthaline-rose, thiomelic acid, and fluor spar.

The difference in luminous intensity between the maxima and minima in the fluorescence-spectra varies a great deal: the maxima in the case of nitrate of uranium form very neat bands

with sharply defined edges; while with fluoresceine, which gives
a fluorescence identical in appearance with that of the former
solution, similar intermissions are hardly to be distinguished.

It might be supposed that the interruptions in the spectrum
projected upon the surface of a fluorescent substance correspond
to the intermissions of its fluorescence-spectrum, the one being
the determining cause of the other : there are cases in which we
might believe such a coincidence to exist; solutions of lamp-
black, for instance, give five maxima in the spectrum thrown
on their surface, and likewise five maxima in the fluorescence-
spectrum.   But what is observed in a great number of other
cases demonstrates that in no instance have we here to do with a
simple relation.   Fresh solution of chlorophyl gives seven
maxima in the spectrum projected upon its surface, but only two
in its fluorescence-spectrum ; naphthaline-rose gives three very
pronounced maxima in the first case, and no intermission in
the second; on the contrary, nitrate of uranium, which gives no
interruption in the spectrum thrown on its surface, shows two
very marked maxima in the spectrum of its fluorescence.

Intermission in the projected spectrum, as well as in that of
fluorescence, may in some cases, as those of the solutions of
guaiacum, purpurine, orchil, and turnsol, be explained by the
fact that they are mixtures of several colouring-matters.
Nevertheless in other cases, where the colouring substance is a
pure crystallizable body, one can no longer see in mixture the
cause of the intermission.

Stokes's Law.

We have already mentioned, at the opening of this article,
the law of Stokes, which states that the light emitted by fluo-
rescence is never more refrangible than the exciting rays.

M. Lommel has recently contested the accuracy of this law,
on the ground of a series of observations made by him on naph-
thaline-rose and chlorophyl.   My own experiments have con-
trarily led me to recognize the perfect correctness of Stokes's
law in all possible cases.   The other laws which it has been
thought might be introduced into the domain of fluorescence
have not found their verification in these researches.

Influence of the Solvent.

The liquid in which the fluorescent substance is dissolved
exerts sometimes an influence on the nature of the light emitted;
but here, again, no precise rule can be established.   The influ-
ence of the solvent manifests itself in many cases by displace-
ment of the bands of maximum in the projected spectrum ; thus
the solutions of the amide of phthalic acid, of chlorophyl, pur-
purine, &c. are distinguished from the others by the bands being

nearer to the violet. The colour of the fluorescence and the position of the maxima in the fluorescence-spectrum also change in some cases with the nature of the solvent; this is seen, for instance, with the amide of phthalic acid and with solutions of lamp-black. On the other hand, there are cases in which the solvent appears to exert no influence, either on the spectrum thrown on the surface of the liquid or on the fluorescence-spectrum.

Influence of the State of Aggregation.

When a substance is fluorescent in the solid state, does it necessarily follow that it must also be so in the liquid state, and *vice versâ*? The answer to this question differs according to the different substances.

There are substances, as the double cyanide of platinum and barium, which are fluorescent in the solid and not at all in the liquid state. There are others, such as nitrate of uranium, which as solids give a very intense fluorescence, and as solutions give only a very faint one. There are also substances which are very fluorescent in both states of aggregation; it is so with photene, sugar of malt, and tincture of curcuma. Moreover some substances, feebly fluorescent in the solid state, are very strongly so in solution—*e. g.* æsculine, sulphate of quinine, chlorophyl, and the amides of phthalic and terephthalic acids. There are, lastly, substances which are not at all fluorescent in the solid state, while they are so in the liquid condition—for example, naphthaline-rose.

Relation between Phosphorescence and Fluorescence.

It is at present impossible to decide whether phosphorescence and fluorescence are two altogether distinct phenomena, or whether there is an insensible transition from one to the other ; the presumptions, however, are in favour of the latter view.

This question can only be satisfactorily answered when we have succeeded in ascertaining in some substances the persistence of fluorescence, even if for only a very short time. I have tried to obtain this result, but have not succeeded; nevertheless I do not pretend to give this fact as decisive, because my apparatus could only make perceptible a persistence of $\frac{1}{1800}$ of a second or more, while it would be indispensable, for this class of experiments, to operate with apparatus very much more delicate.

I remark further that the fluorescence-spectra with intermission which we have demonstrated for a great number of substances are evidently analogous to the spectra of phosphorescent substances studied and described by M. Edm. Becquerel. This fact would also tend to connect the two classes of phenomena.

Theory of Fluorescence.

I do not intend here to launch into a criticism of the different theories by which it has been attempted to explain fluorescence. I will merely affirm that none of those theories is adequate to account for the immense variety of the phenomena, and that it will be long before any future theory will be so.

The ideas expressed by Stokes are still at present the best basis for an attempt at a theory of fluorescence. Thus we must admit with him that the undulations of the æther which strike the fluorescent body set its molecules in motion and cause it to become self-luminous. In this there is a certain analogy with the acoustic phenomenon of bodies vibrating in unison. In one point, however, the difference is very great; spectral analysis of the light of fluorescence excited by homogeneous light does not give homogeneous light, but an infinity of radiations of different wave-lengths. In this respect fluorescence-spectra approach the spectra of incandescent solids. If the light emanating from a homogeneous substance thus presents an infinite variety of wave-length, this can only be explained, as Stokes has already shown, by the action of forces which are not merely proportional to the first power of the amplitude, and in this way produce undulations for which the period of an oscillation is a function of the amplitude. Undulations of this kind must be admitted in the case of an incandescent solid; for without that we cannot explain to ourselves the continuity of the spectrum. The incandescence of a solid body constitutes, in my opinion, a relatively simple problem, the solution of which it is indispensable to have before we pretend to explain the emission of light by fluorescence. The light emitted by incandescent solids of utterly different natures is, as we know, identical; it is therefore independent of their intimate molecular constitution, which, on the contrary, has a notable influence in the case of fluorescence, and adds to the complication of the phenomenon. It is clear that, besides the theoretical considerations which Stokes was obliged to use in order to justify the law which bears his name, there are more than one point which it is important equally to take account of—not only the molecular constitution of the substances, but also the more or less great mass of the material molecule compared with that of the ether atom which puts it in motion. It is, besides, only by means of a complete theory of fluorescence that any one will arrive at a satisfactory explanation of Stokes's law. What has hitherto been attempted of the kind is only a number of more or less venturesome hypotheses. I affirm only that theories deviating from that law would deserve no credence.

VII. *Notices respecting New Books.*

The Earth a Great Magnet. By ALFRED MARSHALL MAYER, *Ph.D.*,
   Professor of Physics in the Stevens Institute of Technology. London :
   Trübner and Co.

THIS is the report of a lecture delivered before the Yale Scientific
   Club on February 14, 1872, in which the lecturer proposed to
present to his audience " *one prominent truth* in simple and striking
experiments." The truth which is kept steadily before the mind
throughout the lecture is, " that the earth is a great magnet;"
and this truth is developed, step by step, by experiments of the
most conclusive kind, each having been rendered distinctly visible
to the audience by means of the *vertical* lantern, so that the pro-
cesses of magnetizing and demagnetizing, with all the interesting
motions of the needles, were seen projected on a luminous screen
of eighteen feet diameter.

   The lecture itself is a masterly production, and exhibits the re-
sult of much close reading as well as experimental research.
Quotations are given from earlier writers on magnetism, illustra-
tive of the sound knowledge which they possessed ; and as each
experiment illustrative of the lecture is described as well as the ·
apparatus employed in manipulation, the reader is conducted from
a consideration of the most ordinary magnetic phenomena pre-
sented by bar and electro magnets, to that of the same pheno-
mena evolved from terrestrial magnetism. A paragraph selected
from the closing portion of the lecture will fully substantiate this
statement.

   " Now we have finished our experiments ; and what have they
shown ? I have temporarily magnetized a bar of soft iron, by
pointing it towards a pole of our large magnet. I did the same
with the bar and the earth. I permanently magnetized an iron
bar, by directing its length towards the pole of the magnet, and
vibrating it with a blow of a hammer. I did the same with a
bar, struck when pointed towards the earth's magnetic pole. I
have shown you the action of a small magnetic disk on iron
filings placed above and around it. You saw that the earth pro-
duced the same action on the beams of the aurora. I showed
you the action of this disk on a freely suspended magnetic needle,
and pointed out to you the earth's similar action on a dipping-needle
carried over its surface. I have evolved a current of electricity
from a magnet, by cutting with a closed conductor across those
lines in which a magnetic needle freely suspended places its length.
I did the same with the earth by cutting across those lines which
are marked out by the pointing of the dipping-needle. There-
fore, what am I authorized to infer? When the effects are the
same, the causes must be the same ; for according to all the princi-
ples of philosophy, and conformably to that universal experience
which we call common sense, like causes produce like effects."

   To those who are desirous of possessing in a compressed form the

leading facts of terrestrial magnetism, we strongly recommend a perusal of the lecture.

War Department Weather Maps. Signal-Service, United-States Army. Friday, November 22, 1872.

We have received the three maps of the above date, 7.35 A.M. 4.35 P.M., and 11 P.M. Washington time respectively. An improvement has been introduced, particularly in the insertion of the Oceanic Currents with their velocities per hour. The land surface is coloured green, the principal mountain-ranges shown, and elevations above 8000 feet left white. A distinction is made between surfaces above and below 800 feet, the former being darkly shaded, the latter lightly. The ordinary isobars, isotherms, state of weather, &c. are shown as formerly.

One of the latest results of the operations of the American Signal-Office, as reported in the ' New York Herald,' Nov. 16, is the discovery of a vast aërial wave which passed over the States in November. It originated in the Pacific Ocean, and was traced distinctly over the whole breadth of the continent ; so that America as well as Western Europe has a " great November wave." The American storm-warnings in connexion with this wave were issued from two to three days in advance.

The issue of three maps daily is of great advantage, as it contributes so much more efficiently to an acquaintance with atmospheric changes, than by an issue of one map daily. The weather maps of the English Meteorological Office are of great utility ; but this utility would be greatly increased by two additional maps being issued daily, for 4 P.M. and midnight. Probabilities, as on the American plan, could then be deduced with more or less success, either officially or privately by subscribers.

The Atmosphere of the Sun. The Rede Lecture, 1871. *By* J. NOR-MAN LOCKYER, *F.R.S.* London : Macmillan and Co.

This Lecture contains a *résumé* of the work effected by Mr. Lockyer in connexion with Dr. Frankland, relative to their joint researches on Solar Physics, an account of which has for some time been before the public. The lecture, which is well conceived, is based upon Newton's query of the sun and fixed stars being great earths vehemently hot, from which vapours and exhalations arise and are compressed and condensed by the vast weight and density of their atmosphere.

The remarks of Mr. Lockyer, bearing on the reply to Newton's query, are of somewhat a desponding character, inasmuch as they allude to the more than century and half century that the world has had to wait for the " outcome " of the splendid generalization by which the proof has been obtained, that the sun and fixed stars are undoubtedly " vehemently hot." Nor is his tone more encouraging towards the close of his lecture ; for in speaking of our witnessing the birth of a new science, he says it is one which requires costly apparatus and appliances above the means of most individuals, with thoroughly organized work extending over centuries. How then is the science of the sun to progress ?

VIII. *Proceedings of Learned Societies.*

June 20, 1872.—Sir James Paget, Bart., D.C.L., Vice-President, in the Chair.

THE following communication was read:—

"On Supersaturated Saline Solutions." By Archibald Liversidge, Assoc. R.S. Mines.

There is, perhaps, no necessity to describe in detail the ordinary phenomena presented by supersaturated saline solutions, since they must now be well known to all.

The following series of experiments have chiefly been made upon sodic sulphate; but before citing them, it may, however, not be out of place to briefly allude, *en passant*, to the conclusions drawn by the numerous writers and experimenters upon this subject, since the results of my own experiments are supported by the authority of some of these observers and run counter to that of others.

The theories which have been put forth are, in the main, as follows:—

a. That the crystallization of supersaturated solutions is caused by purely mechanical agencies, such as agitation &c. The principal supporter of this view was Gay-Lussac, who wrote in 1819. It has since been shown to be utterly untenable.

β. That the sudden crystallization is due to some unknown catalytic force. Advocated by Lowell in 1850, but since disproved.

γ. That it is due to the entrance of a particle of the same salt. This explanation is favoured by the majority of the writers upon the question, such as Ziz in 1809, Gernez in 1851, Violette in 1860, Dubrunfaut in 1869, by Lecoy de Boisdandran, and others.

δ. That crystallization is due to the presence of fatty, oily, greasy, or other matters in the form of thin films. This theory was propounded by Mr. Tomlinson in two papers* read before the Royal Society, in which also it is stated that certain liquids, such as absolute alcohol, act as nuclei in determining the solidification of such solutions by separating water from the solution, whereas the thin film, on the contrary, owes its activity to the greater attraction which it has for the salt held in solution.

Preparation of the Supersaturated Saline Solution.

A little water is placed in the flask, boiled, and sodic sulphate added to the boiling liquid until it ceases to dissolve any more and a deposit of the anhydrous salt begins to take place; the solution is then filtered and transferred to smaller flasks, usually of about 2 oz. capacity; these are then again boiled up after being covered with a small beaker, watch-glass, or plugged with cotton-wool. By this method any nuclei adhering to the watch-glass, beaker, or wool are rendered inactive, even should they fall into the solution.

The solutions are always used of such a degree of supersaturation that crystals of the anhydrous salt are deposited during the boiling.

* Phil. Trans. vol. clviii. pt. ii. and vol. clxi. pt. i.

*Do some liquids, such as alcohol, act as nuclei by combining with a
    portion of the water of the solution and liberating a little salt
    which acts as a nucleus?*

Exp. Supersaturated solutions of sodic sulphate were prepared,
in the manner described, in 2-oz. flasks, which were closed with a
plug of cotton-wool through which a bulb-
tube was passed, of the form figured *, con-
taining absolute alcohol.

After waiting some time to be certain
that nuclei had not gained admittance, some
of the alcohol was run out on to the surface
of the solution by momentarily loosening the
stopper.

This experiment was repeated many times,
at different temperatures and with alcohol of
various strengths ; but never did the alcohol
act as a nucleus.

Previously to the experiment the alcohol
had been boiled to destroy nuclei.

Exp. Concentrated sulphuric acid was
substituted for the alcohol, but likewise with
no result. The smallest quantity of acid was
added, so as to prevent any undue rise in
temperature, which would of course vitiate
the result.   The flask was likewise kept
cold by a stream of water.

In a later form of these experiments, a
small glass bulb with a long neck blown
from glass tubing, such as is used in the
elementary analysis of a fluid by com-
bustion, was made use of.

The bulb was first well heated in a
Bunsen burner, so as to destroy any
nuclei which might adhere to it; then,
while still hot, the open end was dip-
ped into the alcohol or acid under trial,
when, of course, as the air in the bulb
cooled some of the liquid was forced
up into it ; its liquid contents were
then boiled and the open end again
dipped into the fluid ; and as the va-
pour condensed, more fluid was forced
up into it.

The tube was then surrounded by
cotton-wool and inserted into the neck
of the flask, and the supersaturated
solution boiled up for a moment, so
as to render the whole apparatus,
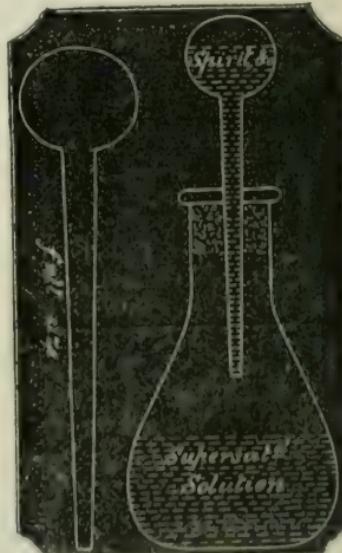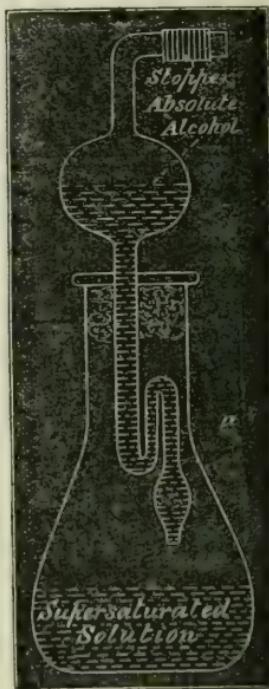
* The loop was made in the tube at *a* so as to prevent any fluid from escaping
until required.

cotton-wool included, inactive, the steam escaping through the inter-
stices of the cotton and not affecting the spirit.   When cold a drop
of the spirit or acid was delivered by merely heating the glass bulb.

Exp. Trial was next made of several solid dehydrating substances,
such as calcic chloride, anhydrous chromic acid, phosphoric anhy-
dride, freshly ignited quicklime, &c.

These bodies were placed in sealed thin glass bulbs and heated
nearly to redness and then dropped into the supersaturated solution ;
the flasks were plugged with cotton-wool, through which a glass rod
passed, and boiled up, after which they were allowed to cool for some
hours ; when quite cold the bulb was broken by means of the glass
rod and its contents set free, but, as in the case of the liquid, with
no result.

It should perhaps here be mentioned that each flask was always
proved to be thoroughly supersaturated by dropping in a crystal of
the salt or touching the solution with a dirty rod, after the sub-
stance made trial of was found to be wanting in nuclear power.

From the foregoing it appears that the crystallization of super-
saturated saline solutions is not determined by the removal of water
by chemical agency; neither do porous bodies, like wood, charcoal,
sponge, spongy platinum, earthenware, &c., determine the solidifi-
cation of solutions by mechanical absorption of the water.

Concerning the action of thin films.

In the same paper it is stated that while oils, fats, and greasy
bodies generally do not act as nuclei when chemically clean and in
the bulk, *i. e.* in the form of a solid mass, lens, or drop, yet these
identical bodies when in the form of thin films do act as nuclei, and
that any substance which possesses a nuclear action has derived
such power from having become contaminated with a thin film of
greasy matter, which it acquires by handling, wiping with a dirty
cloth, or by mere exposure to the air containing the products of
respiration and other excretions, &c.

Thus in the series of experiments detailed it was found that such
bodies as ether, absolute alcohol, naphtha, turpentine, herring-oil,
sperm-oil, castor-oil, and many others, while in the form of a lens
or globule, did not act upon a supersaturated solution, but did im-
mediately when spread out into a thin film.

It should be noticed that the oil was added to the solution by
removing the cover of the flask, delivering the drop, and then repla-
cing the cover ; or a glass tube was used provided with a shield cover-
ing the mouth of the flask : both methods have the great objection
of exposing the solution to the air, and so allowing nuclei to gain
access.

It is stated that if the finger be cleaned by washing it in alcohol
or caustic potash, or by passing it through the flame of a spirit-lamp,
it may be held in a supersaturated solution for some time without
causing crystallization—but that if it be rubbed against the sides of
the flask, a greasy smear is produced which at once acts.

The writer has repeated this form of experiment several times, but

never with the above result when sufficient care had been taken to free the finger from nuclei.

Exp. The finger was made greasy by dipping it into oil and imperfectly wiping it with a cloth ; it was then passed many times through the flame of a spirit-lamp, and finally, while still far above its normal temperature, inserted into a flask of supersaturated solution : the flask was chosen with a neck such that it could be entirely closed by the thicker part of the finger. The flask was then transferred to a vessel of water, lowered artificially to 38° F., and there kept, with the finger still in it, for several minutes, varying in different experiments from 10 to 15, 20, 25, 30, and 35 minutes ; and although the finger was strongly pressed against the sides of the flask, which was seen to be smeared all over, yet crystallization was not set up when the solution was made to flow over the finger-marks, which were plainly visible. That the solutions were not warmed by the heat of the finger, and so rendered inactive, is proved by their immediately solidifying on the insertion of a dirty glass rod.

Exp. By means of the two modifications of bulb-tube already described for the experiments with absolute alcohol, thin films of various oils and other bodies were formed upon the surface of supersaturated solutions without inducing crystallization. That is, a small glass bulb was filled with the oil or other body and boiled, then supported in the neck of the flask by a plug of cotton ; the supersaturated solution was then boiled and allowed to cool ; when quite cold a drop of the liquid was forced out of the bulb on to the solution ; then by a sudden jerk the lens or small globule thus obtained was flattened out into a thin film, often iridescent—but without causing solidification.

In numerous instances the temperature of the solution was lowered by means of ice-cold water, so as to increase its sensitiveness, but with no different result.

In many cases the oil or fatty body, such as olive-oil, Russian tallow, citronella-oil, castor-oil, &c., was dissolved in ether and then used ; this device was used for two reasons :—first, so that the greasy matter might be much diluted and so spread over a large surface, and then be left as a thin film on the evaporation of the ether ; and second, so that a much smaller quantity of the oil might be delivered at a time. Usually the oil collected into globules shortly after the evaporation of the ether, but could generally be spread out into a film again by imparting a sharp twist to the flask.

Supersaturated solutions of sodic sulphate having films of oil, benzole, turpentine, citronella-oil, &c. upon their surface have been kept by the writer for several months together, and some even as long as eighteen months : it is true that the oil &c. soon lost the form of an iridescent film, but could be made to assume it at any moment ; and the above lot of flasks were seldom allowed to stand for a day without being made to do so, *i. e.* for the first three months after their preparation and at greater intervals afterwards. Every now and then a flask was caused to crystallize in order to ascertain that the solutions had in no way lost their sensitiveness to a dirty

rod ; and when the last flask of all was proved, it had stood for rather more than eighteen months.

One explanation accounting for the activity of the thin film as prepared by the eminent author of the paper referred to may be this :— That in order to place the oil upon the solution, the flask was opened and exposed to the air, thus affording an opportunity for nuclei to gain entrance ; and also they may have been carried in by the greasy rod itself, for there would be plenty of time in its passage for it to pick nuclei up : such nuclear bodies would probably float upon the surface of the disk or globules of oil, and would not come into contact with the solution itself; neither might they touch its surface even when the disk was broken up into small globules, for these globules would be immensely large in comparison with the dimensions of the nucleus itself; but, on the other hand, when the disk was flattened out into an iridescent film, and therefore one of excessive tenuity, the nuclei might then easily fall through it, come into contact with the supersaturated solution, and start its crystallization. As it is probable that several nuclei would enter at the same time, they would naturally become dispersed by the jerk, and hence crystallization would be set up at various points.

That nuclei will pass through the substance of a thin film is shown by the solidification which almost immediately takes place on exposing to air the solution covered merely by a film of oil, turpentine, &c. ; a thick coating of oil is, of course, one of the best means we have of protecting a supersaturated solution from nuclei.

The principal substances made use of by the writer for the formation of thin films were as follows :—Citronella-oil, olive-oil, Russian tallow, castor-oil, camphor in alcohol, creosote, turpentine, benzole, chloroform, ether, &c.

Concerning the action of a crystal of the normal sodic sulphate upon a supersaturated solution of the same.

It is well known that there are three modifications of sodic sulphate crystals :—

1. *The anhydrous salt* (Na_2SO_4), crystallizing in octahedra, and deposited from a supersaturated solution on further concentration ; these crystals are inactive to a supersaturated solution.

2. *The modified salt* ($Na_2SO_4, 7H_2O$), containing $7H_2O$, formed in a supersaturated solution by reduction of temperature and other causes ; these also are inactive, and admitted to be so by all.

3. *The normal salt* ($Na_2SO_4, 10H_2O$), crystallizing in prisms with dihedral summits, and containing $10H_2O$. Usually regarded as the best nucleus. Experiments relating to its behaviour as such will be detailed.

It is always the normal salt ($Na_2SO_4, 10H_2O$) which is formed when a solution is caused to crystallize by touching it with a dirty rod or by exposing it to the air, &c.

Experiments were made with recently generated crystals of the normal salt.

Exp. Two beakers, containing fully supersaturated solutions, were

covered with watch-glasses, and allowed to cool; in one of the beakers a small glass bucket, attached to a thread, had been placed and boiled up with the solution. Next, both beakers were arranged under a large bell-jar, and the silk thread from the bucket passed up between the stopper and the neck of the jar. The solutions were then uncovered, after waiting ten minutes for any nuclei which might have been disturbed to fall; a fine wire was passed down into the beaker containing the bucket, and as far as possible from the part of the solution through which it would pass on being drawn up.

The bucket, now full of the crystallized normal sodic sulphate ($Na_2 SO_4$, $10H_2O$), was raised, and lowered into the second beaker of still fluid solution; immediately that the point of one of the crystals hanging from the under surface of the bucket touched the solution, crystallization was set up instantaneously throughout the mass.

This experiment was performed many times, and with every possible care to prevent the entrance of nuclei other than those purposely borne by the wire.

A modification of the above plan was tried and with similar results.

Exp. A tubulated glass bell was fitted with a cork bearing two glass tubes, open below and closed above with cotton-wool; they were bent so as to permit both of them being placed in one and the same beaker, or into either separately.

In the first place, the ends of the tubes inside the bell were freed from nuclei by passing them through a flame; two beakers of cold supersaturated solution were then placed in position under the bell-jar, and their covers removed. After waiting five minutes or so for any dust to settle, both tubes were next lowered into one of the beakers, on opposite sides, so as to be as far apart as possible. A dirty wire was now passed down one of the tubes, when, of course, crystallization immediately took place, and was propagated across the beaker. The second tube, with its adhering crystals, was then raised and lowered into the second beaker, when, the moment the extreme point of the longest crystal touched the surface of the solution, crystallization immediately started from that point, and the whole contents became solid.

A third variation was then made in this experiment. One of the two beakers was replaced by a U-tube of thin, hard glass, one of the before-mentioned tubes being inserted into either limb. Crystallization, when set up in one limb, travelled round the bend and up into the other, from which crystals were transferred, as before, to a beaker or flask of solution also under the bell-jar.

The three modifications of this form of experiment were tried time after time, and always with the same unvarying result. Solutions which were supersaturated although not perfectly, and therefore less sensitive, were operated upon in this way; but, even with such less favourable circumstances, the normal crystals always started crystallization in the solution to which they were added.

To ascertain, if possible, whether nuclei, other than crystals of the normal salt, were carried by the tube or its adhering crystals, a

capsule of sulphuric acid was placed under the bell. The crust of crystals was by this means dried, and became effloresced to a greater or less extent. Now, on lowering them into a supersaturated solution of alum or of magnesic sulphate they were proved to be inactive, having been changed to the inactive anhydrous salt.

But such dried normal crystals were active to a solution of sodic sulphate, even after three days' exposure to the sulphuric acid[*]. It seems as if the normal crystals become covered with a coating of effloresced anhydrous salt, which acts as a protection to the underneath portions in the same way as oxide of lead does to metallic lead; hence it takes a long time to convert a crystal of the normal salt into the anhydrous by simple exposure to dry air, although it is an exceedingly short operation to perform at temperatures superior to 34° C.

Yet another form of this experiment was tried again and again, and always with the same result.

A glass tube bent into the form of an elongated letter S was suspended by a plug of cotton-wool in the neck of a flask containing a supersaturated solution; the solution was boiled, and the tube was also boiled in it, so as to get all nuclear particles adhering to it thoroughly destroyed.

The solution was then allowed to cool, with the tube still in it; the tube was then raised out of the solution and a dirty wire passed down it; crystallization was, of course, set up in the portion of supersaturated solution contained within the tube; the crystals gradually grew down the tube, then through the first bend, travelled up the upright portion, then travelled round the second bend, and finally down the third and last straight portion. Now, on lowering the extreme tip of the crystals formed at the end of the tube into the solution, crystallization was immediately set up from it as a centre, and thence throughout the mass.

Supersaturated Solution

By this arrangement access of extraneous nuclei was entirely prevented. The upper end of the tube was plugged with cotton-wool until the dirty wire was passed down.

That the normal crystals thus formed did not act by any transient molecular movements, which recently formed crystals might be supposed to have, is proved doubtless by the fact that such crystals were found to act just as readily even when they had been kept over the solution for $2\frac{1}{2}$, 5, 10, 24, and 48 hours, and then lowered into the solution, and when any molecular agitation may with fairness be supposed to have ceased.

[*] At a future day I hope to have the results of more experiments upon this point.
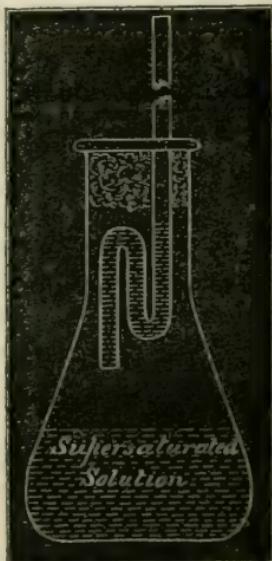
Exp. Supersaturated solutions of common potash alum were treated in the same way and with the like results ; alum, perhaps, affords a prettier example even than sodic sulphate, since the crystals formed in the tube are of an opaque white, and can therefore be more readily observed during their growth.

Exp. Supersaturated solutions of magnesic sulphate were also operated upon and with the same success ; but the experiment is not so striking, owing to the much longer time required by magnesic sulphate to crystallize.

Although pure clean crystals of the normal sodic sulphate are active to a supersaturated solution of sodic sulphate, yet, as might be expected, they are not active to a similar solution of alum or magnesic sulphate, and *vice versâ*.

For example, let us take a supersaturated solution of alum, and one of sodic sulphate, and also crystals of both their salts, which crystals have just formed and are taken from their still warm mother-liquors.

Exp. A crystal of alum from its mother-liquor was added to a supersaturated solution of alum. Crystallization immediately took place.

Exp. A like crystal of alum was then added to a supersaturated solution of sodic sulphate. No effect.

Exp. A crystal of the normal salt was taken from its mother-liquor and added to a solution of sodic sulphate. The solution instantly crystallized, although another crystal was inactive to a solution of alum.

Exp. A crystal of magnesic sulphate was added to solutions of alum and of sodic sulphate respectively. No effect on either, but active in a solution of magnesic sulphate.

Concerning the composition of the crystals of sodic sulphate formed by spontaneous evaporation of a supersaturated solution of the same.

When a supersaturated solution of sodic sulphate is allowed to evaporate spontaneously, a crust or ring of crystals forms on the surface of the solution, or a ring in the upper part of the vessel ; these crystals are perfectly inactive, as has long been known ; and this has been accounted for by regarding them as crystals of the modified salt ($Na_2SO_4, 7H_2O$), which is non-nuclear : but recently they have been regarded as crystals of the normal salt ($Na_2SO_4, 10H_2O$), and their want of action upon the supersaturated solution has been explained by saying that, unlike crystals which have been exposed to the air, they are chemically clean, and therefore free from any film of greasy or other matter ; for this writer views the activity shown by the normal salt as being entirely due to impurity of this kind, and not as due to any property inherent in it.

Löwel made analyses of this salt, formed by spontaneous evaporation, and found it to consist of the modified salt containing $Na_2SO_4, 7H_2O$.

Faraday also examined it and came to much the same conclusion, only that he gave it $8H_2O$, instead of $7H_2O$. There is no doubt that Faraday obtained this salt and not the normal with $10H_2O$,

although he made what has since been proved to be a mistake in assigning $8H_2O$ to the modified salt.

The writer allowed some supersaturated solutions of sodic sulphate to evaporate spontaneously, and, after several vain attempts, at last succeeded in obtaining good crops of such crystals, without admixture of the normal salt, which, of course, is liable to crystallize out also on opening the receiver. The ring of crystals at the top of the solution only were taken.

Results of determinations of water of crystallization in crystals of sodic sulphate formed by spontaneous evaporation.

No. 1. $\cdot365$ grm. of salt, on drying in water-oven at 100° C., after first well drying the powdered salt with blotting-paper, lost $\cdot170$ grm. $= 46\cdot57$ per cent. $OH_2, = Na_2SO_4 7OH_2$.

No. 2. $\cdot172$ grm. lost $\cdot081$ grm. $= 47\cdot09$ per cent., $= Na_2SO_4, 7OH_2$.
No. 3. $2\cdot708$ grms. lost $1\cdot273$ grm. $= 47\cdot00$ per cent., $= Na_2SO_4, 7OH_2$.
No. 4. $1\cdot260$ grm. lost $\cdot605$ grm. $= 47\cdot00$ per cent., $= Na_2SO_4, 7OH_2$.
No. 5. $3\cdot936$ grms. lost $1\cdot812$ grm. $= 46\cdot69$ per cent., $= Na_2SO_4, 7OH_2$.
No. 6. $3\cdot275$ grms. lost $1\cdot520$ grm. $= 46\cdot41$ per cent., $= Na_2SO_4, 7OH_2$.
No. 7. $3\cdot326$ grms. lost $1\cdot570$ grm. $= 47\cdot11$ per cent., $= Na_2SO_4, 7OH_2$.

No.	Weight. grms.	Loss. grm.		OH_2. per cent.
1.	$\cdot365$	$\cdot170$	$=$	$46\cdot57$
2.	$\cdot172$	$\cdot081$	$=$	$47\cdot09$
3.	$2\cdot708$	$1\cdot273$	$=$	$47\cdot00$
4.	$1\cdot260$	$\cdot605$	$=$	$47\cdot00$
5.	$3\cdot936$	$1\cdot812$	$=$	$46\cdot69$
6.	$3\cdot275$	$1\cdot520$	$=$	$46\cdot41$
7.	$3\cdot326$	$1\cdot570$	$=$	$47\cdot11$

I trust that by the above-mentioned results I have clearly proved the following facts with respect to supersaturated solutions of sodic sulphate :—

1. That liquids and solids, such as alcohol, quicklime, &c., do not determine crystallization by removing water.

2. That thin films, when sufficient precautions are taken to guard against the entrance of nuclei, do not act as nuclei.

3. That chemically clean crystals of the normal salt ($Na_2 SO_4$, $10H_2O$) *do act* as nuclei and are most powerful.

4. That crystals of the normal salt are not produced in supersaturated solutions of sodic sulphate on allowing it to evaporate spontaneously, but that crystals of the modified (and known inactive) salt are.

In conclusion I may perhaps be permitted to state that the above series of experiments have extended over a period of three years, less a few months, and that most of them have been repeated a countless number of times, and with every conceivable modification and check. Some few of them have already been published in the 'Chemical News,' but are here referred to again for the sake of comprehensiveness.

At present the writer does not venture to put forth any definite theory respecting the presence and nature of the nuclei which are so universally diffused throughout the atmosphere; but when it is considered how much sodic chloride is constantly present in the air, and what quantities of sulphurous acid are evolved daily, which becomes partly converted into sulphuric acid, the presence of particles of sodic sulphate in the air would not be surprising; and that it does exist is proved by drawing air through water and finding comparatively large quantities in the solid matter arrested by water.

Sodic sulphate solutions, too, crystallize on exposure much more readily than those of any other salt. The other salts which form supersaturated solutions are certainly less diffused than sodic sulphate.

IX. *Intelligence and Miscellaneous Articles.*

ON THE DISTRIBUTION OF MAGNETISM. BY M. JAMIN.

AT the last Meeting of the Academy, M. Trève communicated a note on magnetism, in which he states, among other things, that the poles of a magnet are displaced, being removed from the extremities, when an armature of soft iron is applied. He believes he demonstrates this by placing opposite to the magnet a magnetized needle, by showing that its direction changes after the application of the contact, and by supposing that the pole is in the prolonged direction of the needle.

We ought first to define the word *pole*; we must then remark that, in this experiment, the magnetized needle is submitted to the attractive action of the steel which forms the magnet, of the iron which constitutes the contact, and, again, to the attraction or repulsion of the free magnetism of the magnet. Its direction is along the resultant of these actions—a resultant evidently very complicated, depending on the distance, the weight, and the form of the contact : it is clear that the direction of this resultant cannot lead to any precise conclusion.

What it is necessary to determine here is the distribution of the magnetism in the magnet, both before and after the application of the contact; this is a question with which I have long occupied myself, and on which I will now say a few words.

I study this distribution by two mutually corroborative processes. The first consists in placing upon a point of the magnet a small electromagnet of soft iron enveloped in copper wire which communicates with a galvanometer, and suddenly pulling it away. An induction-current is produced, a galvanometric deflection ; and from the magnitude of the arc of impulsion I can calculate, after a suitable graduation, the magnetic intensity at the point touched.

The second process, analogous to Coulomb's, consists in placing on the point one wishes to study a small sphere of soft iron supported by a spring, which is stretched progressively till separation takes place. Its tension at that moment is proportional to the square of the magnetism at the point of contact.

If the magnet has its extremities free, it is ascertained that the free magnetism increases gradually from the neutral line to the extremities. The curve of the intensities is very near that indicated by Coulomb. When a contact is applied, all is changed : two poles make their appearance at the two extremities of the contact ; the free magnetism of the magnet partially disappears ; it augments with the distance to a maximum, to decrease afterwards towards the middle line of the magnet. The conditions of these modifications are very complicated, and merit a very close study. They contribute to the production of the changes of direction of the needle examined by M. Trève ; but in no case can these changes reveal the distribution of the magnetism in the bar, and they cannot be summed up by saying that the poles of the magnet have changed their position.—*Comptes Rendus de l'Académie des Sciences*, vol. lxxv. pp. 1572, 1573.

RELATION BETWEEN THE PRESSURE AND THE VOLUME OF SATU-
RATED AQUEOUS VAPOUR WHICH EXPANDS IN PRODUCING
WORK WITH NEITHER ADDITION NOR SUBTRACTION OF HEAT.
BY H. RESAL.

Let V, p, ρ, r be the volume, pressure, density, and heat of volatilization of saturated aqueous vapour at t_0, and c the specific heat of water at the same temperature. Admitting that the suffix 0 refers to a determined weight of saturated dry vapour, and the suffix 1 to the vapour not condensed during the expansion, we have, on properly transforming one of Clausius's equations :—

$$\frac{V_1}{V_0} = \frac{273+t_1}{r_1}\frac{\rho_0}{\rho_1}\left(\frac{r_0}{273+t_0} - 2\cdot30258\frac{c_0+c_1}{2}\log\frac{273+t_1}{273+t_0}\right).$$

I have considered some values of t_0, successively decreasing by 10 degrees, from 200 to 110 ; for each of them I diminished t_1 10 degrees, commencing at $t_0 - 10$. I was thus able to form Tables giving values of $\frac{V_1}{V_0}$, opposite to which I placed the corresponding values of $\frac{p_0}{p_1}$; and I found that the relation

$$\frac{p_0}{p_1} = \left(\frac{V_1}{V_0}\right)^{t\,.\,133}$$

very satisfactorily accords with the elements of those Tables, between the limits 1·25 and 15·37 of $\frac{V_1}{V_0}$.—*Comptes Rendus de l'Acad. des Sciences*, Dec. 2, 1872, p. 1475.

ON THE DEFINITION OF TEMPERATURE IN THE MECHANICAL
THEORY OF HEAT, AND THE PHYSICAL INTERPRETATION OF
THE SECOND FUNDAMENTAL PRINCIPLE OF THAT THEORY. BY
E. MALLARD.

The study of calorific phenomena has led to the introduction into science of two quantities *sui generis*, the calorie and the temperature, which every theory must necessarily define.

Thermodynamics defines the calorie very clearly ; it only indirectly defines the temperature, by means of Carnot's theorem : hence the difficult and embarrassed course of the theory.   I have proposed to myself to remedy this defect.

In the first part of the work which I have the honour to submit to the Academy I demonstrate that Carnot's theorem is identical with the following :—*The mean vis viva of an atom forming part of a body, the absolute temperature of which is* τ, *may be expressed by* aτ, a *being a specific coefficient which can only be dependent on the nature of the atom.*

The following is a compendium of the demonstration of this proposition :—

If we produce a change in the elementary state of a body submitted at every instant to external forces which balance the internal forces, we shall have

$$d\mathfrak{T} + \mathrm{E}d\mathrm{Q} = d\Pi + d\Phi + d\Psi,$$

$d\mathfrak{T}$ being the variation of the external work, $d\mathrm{Q}$ the quantity of heat supplied to the body, $d\Pi$ the variation of the potential function corresponding to the displacement of each atom from its mean position, $d\Phi$ the variation of the actual energy, and $d\Psi$ the variation of that part of the potential energy which depends on the vibratory motion.   On account of the constant equilibrium between the external and internal forces, we have

$$d\mathfrak{T} = d\Pi,$$

and there remains

$$\mathrm{E}d\mathrm{Q} = d\Phi + d\Psi.$$

Instead of determining, according to custom, the condition of a body at any instant by two quantities such as the pressure p and the volume v, I suppose it determined by two auxiliary variables F and R, chosen so that

$$\mathrm{FR} = 2\Phi, \quad \mathrm{F}d\mathrm{R} = d\Psi.$$

The lines of equal energy will then be equilateral hyperbolas

$$\mathrm{FR} = 2\Phi. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

The adiabatic lines satisfy the equation

$$\mathrm{F}d\mathrm{Q} = \mathrm{F}d\mathrm{R} + d\Psi = 0,$$

or

$$\mathrm{FR}^? = \text{const.} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Equations (1) and (2) having the same forms as the equations of isothermic and adiabatic lines for perfect gases, analogous consequences are deduced from them—namely, that if Q_0 is the quantity of heat supplied to the body, in a cycle analogous to that of Carnot, along the line of equal energy Φ_0, and Q_1 the quantity of heat expended by the body during that cycle along the line of equal energy Φ_1.

$$\frac{Q_0}{Q_1} = \frac{\Phi_0}{\Phi_1}.$$

Let us suppose Q_0 and Q_1 infinitely small, as well as the arcs of the lines of equal energy corresponding to them. Let us draw, through the intersection of the line of equal energy Φ_0 with the adiabatic line on the left, an infinitely small arc of an isothermal curve stopping at the adiabatic line on the right; and let us make the same construction for the point of intersection of the curve of equal energy Φ_1 and the left adiabatic line. The quantities of heat Q_0' and Q_1' which must be respectively supplied to and withdrawn from the body along the isothermal arcs thus traced will, in virtue of Carnot's theorem, be connected by the relation

$$\frac{Q_0'}{Q_1'} = \frac{\tau_0}{\tau_1},$$

τ_0 and τ_1 being the absolute temperatures corresponding to the isothermal lines.

Now it is easily demonstrated that Q_0 and Q_0', Q_1 and Q_1', respectively differ only by infinitely small quantities of the second order. We have therefore

$$\frac{Q_0}{Q_1} = \frac{Q_0'}{Q_1'} = \frac{\tau_0}{\tau_1} = \frac{\Phi_0}{\Phi_1},$$

from which, finally, is deduced the general relation between the mean actual energy Φ and the absolute temperature τ,

$$\Phi = \beta\tau,$$

β being a specific coefficient depending only on the nature of the body.

This equation, true for any body or portion of a body whatever, is also true for an atom; hence the theorem stated above.

In the second part, I demonstrate that if, as experiment indicates, the condition of a body is determined when the atomic arrangement produced by a certain equilibrium between the external and internal forces is known and a single quantity, the temperature, is given, this arises from the circumstance that the atoms, instead of merely being in the presence of each other, are under the influence of the æther—a fluid whose vibration-period has an extremely small duration in comparison with that of the atomic vibration.

I show that the $3n$ expressions which determine the mean *vires vivæ* of the n atoms constituting any body whatever contain $3n$ arbitrary constants, so that the *vires vivæ* are independent of the arrangement and mutual forces of the atoms. They are dependent, therefore, only on the action of the æther; so that the atoms of the body vibrate as if, the body being completely disaggregated, they, immersed in the æther, no longer exerted any action upon one another.

I hence infer, by very simple considerations, that, if ϕ represents the mean *vis viva* of an atom, or of the centre of gravity of any

system of atoms in equilibrium of temperature, we have

$$\alpha\phi = f(\tau), \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

α being a specific coefficient, and $f(\tau)$ a function of the absolute temperature, which is the same for all the atoms or systems of atoms.

Lastly, from the value, ascertained by experiment, of the *vis viva* of the centre of gravity of the molecule of any perfect gas, I deduce that α is the same for all the atoms or atom-systems, and that equation (3) may be put in the form

$$\phi = \alpha\tau,$$

α being a constant which plays, in regard to the temperature, a part analogous to that of the mechanical equivalent in regard to the calorie.

This is, with one more degree of generality, the consequence which I deduced from Carnot's theorem, which is thus, I think, for the first time rationally demonstrated.

Ampère, in a remarkable note inserted (in 1833) in vol. lviii. of the *Annales de Chimie et de Physique*, had indicated some ideas analogous to those I have developed in my memoir.

In another paper I shall point out some of the most important consequences which may be deduced from my theory. I shall particularly dwell on those which have drawn upon the theory of the atomic volumes of bodies—a theory of great importance for chemistry and mineralogy, and the study of which was the starting-point of my researches in thermodynamics.—*Comptes Rendus de l'Acad. des Sciences*, vol. lxxv. pp. 1479–1482.

ON ELECTROMAGNETISM.   BY M. TRÈVE.

It is known that steel needles can be magnetized by discharges from a Leyden jar passing through a wire running round them; consequently static electricity has in this case the same properties as dynamic currents. This property belongs also to Ruhmkorff's induction-coil when its currents are caused to pass in a *glass solenoid* containing a rarefied gas.

I placed in the axis of the solenoid some bars of steel, which a single discharge was sufficient to transform into magnets. This was a thing foreseen. But though this experiment establishes no new fact, it is perhaps of a kind to make comprehensible the phenomenon by means of which Ampère completed his theory of terrestrial magnetism. Indeed, suppose we make the glass solenoid big enough to envelop the earth, and cause currents from an induction-machine to pass through it, we shall magnetize every thing magnetic found there, and all needles will be deflected in proportions variable with the intensity of the current.

I think we have here a tolerably correct picture of the electrified gaseous currents which, according to Ampère, incessantly circulate round our globe from east to west, forming as it were an immense coil that acts on the variable horizontal and vertical currents.— *Comptes Rendus de l'Acad. des Sciences*, vol. lxxv. p. 1624.

X. *On the Spectrum of the Bessemer-flame. By* W. Marshall Watts, *D.Sc., Physical Science Master in the Giggleswick Grammar School*[*].

[With Two Plates.]

IN the Philosophical Magazine for December 1867 I published the results of observations on the spectrum of the Bessemer-flame, made with a one-prism spectroscope on the London and North-Western Company's Works at Crewe. These experiments showed that the Bessemer-spectrum contained, besides the lines of potassium, sodium, and lithium, certain lines due to iron; but most of the lines were not found to be coincident with the known lines of carbon or of any other element. Nevertheless I held strongly the opinion that the spectrum was mainly due to carbon, for the following reasons :—

(*a*) Carbon is known to give more than one spectrum (Phil. Mag. Oct. 1869, Chem. News, Oct. 1870); and though the Bessemer-spectrum does not coincide with any recognized spectrum of carbon, it is yet observed in the flame of burning coke, and in other cases where carbon would seem to be the essential element present.

(*b*) The spectrum disappears almost precisely at the right moment for stopping the blast, which is supposed to be when the carbon in the pig iron has been burnt out and the iron is in the condition of molten wrought iron.

I have now to give the results of further observations on the spectrum of the Bessemer-flame, which show that the lines are

* Communicated by the Author.

mainly due to *manganese oxide* (not, as has been stated, to *manganese*, no single line of whose spectrum coincides with a Bessemer line). These observations were made at the works of the Barrow Hæmatite Steel Company at Barrow ; and I am glad to have this opportunity of expressing my sense of the liberality with which the Directors of the Company assisted a purely scientific investigation, not only by affording every facility for the carrying out of experiments, but even by contributing to the expense of the investigation.

The instrument employed was Browning's automatic spectroscope of six prisms. Most of the measurements were made by means of the micrometer tangent-screw of the instrument, which requires 2·94 turns to carry the cross-wires of the telescope from the lithium-*orange* line (wave-length 6101) to the least-refrangible D line (wave-length 5895). In some of the later measurements much more exact results were obtained by the use of a micrometer eyepiece furnished with two pairs of cross-wires. With this instrument the interval between the same two lines is represented by 12·49 turns of the micrometer-screw. The spectrum was mapped throughout on the scale of wave-lengths, the wave-lengths of the lines of the Bessemer-spectrum being obtain. d by interpolation from the wave-lengths of the known lines of some metal, whose spectrum was arranged so as to be visible together with the Bessemer-spectrum.

It was found, however, that (in consequence, probably, of the complicated motion of the prisms in the automatic arrangement) the observing-telescope did not always travel at exactly the same rate, so that the interval between two given lines was not always represented by the same reading. For example, in twelve successive measurements of the interval between the most refrangible D line and the least refrangible *b* line the following numbers were obtained :—

14·11,   13·71,   13·73,   13·68,   13·69,   13·77,   13·77,
     13·74,   13·77,   13·78,   13·75,   13·78.

But although the rate of motion varies, yet in any one passage of the telescope (and prisms) from the red end to the blue end of the spectrum the motion is uniform, so that the wave-length of any line in the Bessemer-spectrum is correctly obtained by interpolation from the wave-lengths of two known lines between which it lies. Accordingly in each " blow " the spectrum was mapped steadily from the red end to the blue end, the tangent-screw being kept firmly clamped and the telescope never being allowed to return upon itself. The readings obtained in each blow were then reduced to wave-lengths by means of one interpolation-

curve; and although the curves obtained vary slightly in degree of curvature, they yield concordant results.
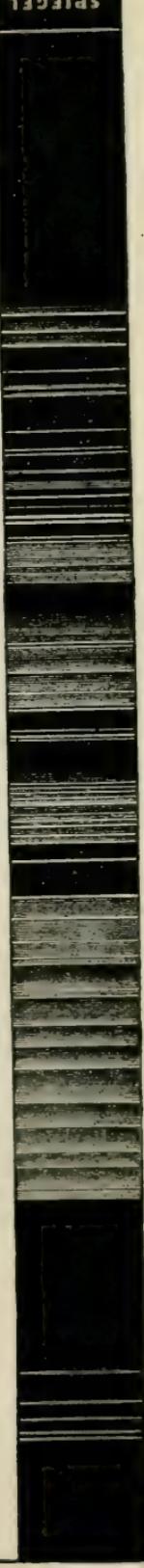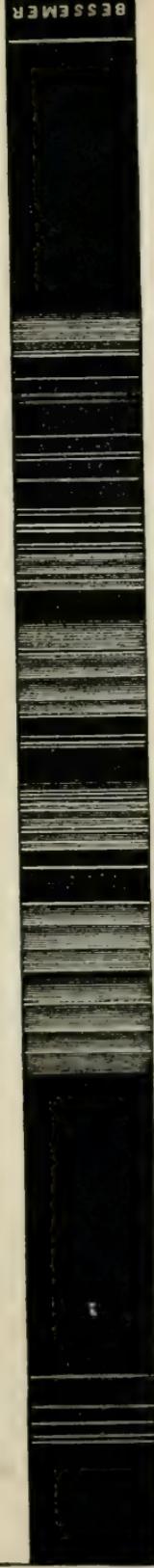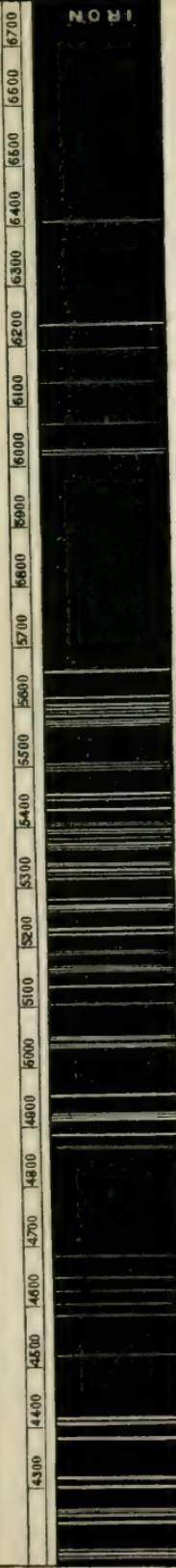
The following determinations of the wave-lengths of two of the brightest lines will show about the degree of accuracy attainable :—

	Reference-spectrum.		Bessemer-spectrum.	
	Wave-length.	Reading.	Reading.	Deduced wave-length.
Air . . .	5678	11·33		
	. .	. .	13·18	5580
Air . . .	5534	14·09		
	. .	. .	17·73	5359
Copper . .	5292	19·24		
Copper . .	5217	21·00		
Lead . .	5607	9·77		
	. .	. .	10·28	5580
Lead . .	5372	14·55		
	. .	. .	14·84	5358
Air . . .	5495	12·21		
Lead . . .	5372	14·63		
	. .	. .	14·89	5360
Lead . . .	5045	22·50		
Cadmium .	5378	14·38		
	. .	. .	14·81	5358
Cadmium .	5337	15·27		

The following measurements of the same two lines were made with the micrometer eyepiece :—

	Reference-spectrum.		Bessemer-spectrum.	
	Wave-length.	Reading.	Reading.	Deduced wave-length.
Tin . . .	5563	0·10		
	. .	. .	1·55	5580
Tin . . .	5588	2·20		
Cadmium .	5338	0·10		
Thallium .	5348	1·28		
	. .	. .	2·22	5359
Cadmium .	5378	3·72		
Tin . . .	5347	0·10		
	. .	. .	1·12	5359
Tin . . .	5368	1·88		

The observatory was placed against the wall of one of the sheds, about on a level with the top of the convertors, close to two convertors, and commanding a distant view of two others. The distant convertors were found to be the best for careful measurement, the shaking being less than when the blow was taking place at one of the near convertors, although the spectrum from these was, of course, the most intense. The best method of introducing the reference-spectrum was found to be, to throw an image of the Bessemer-flame upon the slit by means of a large lens of about 10 inches focus, and to bring the spark-discharger (or Bunsen burner) between the lens and slit. A screen was arranged so as to cut off the light of the Bessemer-flame when required, so that either of the two spectra could be obtained alone at pleasure, or the one could be superposed on the other.

The metals employed to furnish the reference-lines were the following, besides which the lines of the air-spectrum were made use of:—aluminium, copper, cadmium, iron, lithium, lead, magnesium, manganese, platinum, sodium, thallium, tin, and zinc. Further, the Bessemer-spectrum was carefully *compared* with various spectra, especially with those of iron, sodium, lithium, manganese, and manganese oxide. The spectra were either arranged under the Bessemer-spectrum by the use of the reflecting prism, or were superposed on it in the manner described above. The spectra of iron and manganese were obtained by taking the electric spark between wires of these metals—the spectra of sodium, lithium, and manganese oxide by the use of a Bunsen-burner, or by heating the substance in the flame of the oxyhydrogen blowpipe. When manganese chloride (or manganese carbonate or pyrolusite) is heated in the oxyhydrogen flame a very brilliant spectrum is obtained, which, as will be seen afterwards, is for the most part coincident with the Bessemer-spectrum. Observations were, further, made on the spectrum of the flame obtained on adding the Spiegeleisen, on the temperature of the flame at different stages in the process, and on the differences in the spectrum caused by the employment of different iron. The results obtained will be detailed in succession.

I. *Measurement of the Wave-lengths of the Lines in the ordinary Bessemer-spectrum, in the Spiegel-spectrum, and in the spectrum of Manganese Oxide* (Plate IV.).

Bessemer.	Spiegel.	Manganese oxide.	Remarks.
6560 ?	6560	......	} Two red lines, not always seen, estimated position only.
6460 ?	6460	......	
6234	6234	6234	Fine line.
6218	......	......	Fine line; occurs only in Bessemer.
α { 6204	6204	6204	Most refrangible edge of band, shading away (like all the bands in the Bessemer-spectrum) towards the red. Appears in the Spiegel as a bright line, and in the manganese-oxide spectrum as the *least-*refracted edge of a very narrow band. See figure (Pl. IV.).
6185	......	6185	Fine line.
6178	6178	6178	Edge of band. The remark to 6204 applies also to this.
6161	6161	......	Fine line, faint.
β { 6109	6109	......	} Conspicuous pair of red lines, absent in manganese-oxide spectrum.
6097	6097	......	
		{ 6080 } 2 6060 }	Four faint lines, too strong in drawing; position estimated only.
6040	6040	......	Faint line.
6012	6012	......	} Pair of faint lines.
6006	6006	......	
γ 5972	5972	......	Strong line, absent in the manganese-oxide spectrum.
......	5946	......	Four equidistant lines, not very bright, of which the most refrangible read 5946.
......	5932	5932	Line. In manganese-oxide spectrum only, and sometimes in Spiegel.
5917	......	......	
5909	......	5909	In Bessemer-spectrum as a faint line; in the manganese-oxide spectrum as the edge of a band.
{ 5895	5895	......	} Sodium lines.
{ 5889	5889	......	
5872	5872	......	} Two faint lines, absent in the manganese-oxide spectrum.
5865	5865	......	
δ { 5847	5847	5847	Maximum of light.
5819	5819	......	Brightest edge of band, fading away towards the red.
5807	5807	......	Fine line.
5790	5790	......	Strong fine line, forming the brightest edge of the whole group.
5705	......	......	Fine line.
ε { 5688	5688	5688	Splendid double line, conspicuous among the fine lines which make up the whole of this group, nearly coincident with Na β { 5687 5681.
5683	5683	5683	
5644	5644	5644	Brightest edge of band, shading off into red.
5607	5607	5607	Brightest edge of band.
5580	5580	5580	Edge of band, hazy in the manganese-oxide spectrum, but sharply defined in the Bessemer and Spiegel.

Dr. W. M. Watts *on the Spectrum*

Table (*continued*).

Bessemer.	Spiegel.	Manganese oxide.	Remarks.
ζ { 5547	{ 5547	... ..	Group of three lines, the middle one the strongest. In Bessemer and Spiegel only.
5532	5532	......	
5529	5529	......	
(5462	(5462	......	Three faint lines, absent in the manganese-oxide spectrum.
{ 5454	{ 5454	......	
(5443	(5443	......	
5433	5433	5433	Fine line. [red.
5423	5423	5423	Brightest edge of band, fading off towards
η { 5405	......	......	Line.
5395	5395	5395	Strong line.
5391	5391	5391	Edge of band.
5371	5371	......	Strong line, absent in the manganese-oxide spectrum.
(5359	5359	5359	Bright edge of this group, which fades away towards the red.
5327	5327	......	Strong line.
5269	5269	......	Strong line, coincident with the solar line E.
(5229	5229	5229	Edge of band.
θ { 5192	5192	5192	Edge of band. [trum.
5167	5167	......	Line, absent in the manganese-oxide spec-
(5157	5157	5157	Edge of band. [trum.
(5107	5107	......	Line, absent in the manganese-oxide spec-
5099	5099	5099	Edge of band.
ι { 5052	5052	5052	Edge of band.
5018	5018	5018	Edge of band.
(4984	4984	4984	Edge of band.
......	4943	4943	Edge of band.
......	4904	4904	Edge of band.
......	4862	4862	Edge of band.
......	4836	4836	Edge of band.
......	4802	4802	Line.
......	4783	4783	Line, forming edge of band.
(4481	4481	......	Line.
4432	4432	......	Line.
κ { 4404	4404	......	Line.
4383	4383	......	Line.
(4373	4373	......	Line.

Of the Bessemer lines which do not occur in the manganese-oxide spectrum, the following are due to iron:—

5371, 5327, 5269, 5192, 5107, 4383.

The line at 5167 is also nearly coincident with the double iron line $\begin{cases} 5167 \\ 5168 \end{cases}$; and the edge of the band at 5229 coincides nearly with the double line $\begin{cases} 5226 \\ 5232 \end{cases}$.

Fig. 1, Plate V. shows these coincidences. Those lines only of the iron spectrum are drawn which were actually mapped together with the Bessemer-spectrum, when the two were seen simultaneously.

The same figure shows also the result of a direct comparison of the Bessemer-spectrum with that of manganese oxide : those lines only of the manganese oxide spectrum are drawn which were seen to coincide with the Bessemer (or Spiegel) lines.

It is worthy of remark that the identification of these iron lines proves that iron may exist as vapour at a temperature below the melting-point of iron, since the Bessemer-flame is not hot enough to melt wrought iron. The presence of a few only out of the 180 lines which constitute the spectrum of the electric spark between iron poles, is only the same thing that we observe in the case of sodium, lithium, and thallium when the spectrum is produced at a low temperature.

The following lines in the Bessemer-spectrum remain un-identified :—

$$
\begin{array}{ll}
\left.\begin{array}{l} 6560 \\ 6460 \end{array}\right\} & \text{Red lines not always seen.} \\[4pt]
\left.\begin{array}{l} 6218 \\ 6161 \end{array}\right\} & \text{Fine lines.} \\[4pt]
\beta \left\{\begin{array}{l} 6109 \\ 6097 \end{array}\right. & \text{Strong red lines.} \\[4pt]
\quad 6040 & \text{Faint line.} \\[4pt]
\left.\begin{array}{l} 6012 \\ 6006 \end{array}\right\} & \text{Faint lines.} \\[4pt]
\gamma \ 5972 & \text{Strong red line.} \\[2pt]
\quad 5917 & \text{Faint line} \\[4pt]
\left.\begin{array}{l} 5872 \\ 5865 \end{array}\right\} & \text{Faint yellow lines.} \\[4pt]
\delta \left\{\begin{array}{l} 5819 \\ 5807 \\ 5790 \end{array}\right\} & \text{Bright light-green group.} \\[6pt]
\zeta \left\{\begin{array}{l} 5547 \\ 5532 \\ 5529 \end{array}\right. & \\[6pt]
\left.\begin{array}{l} 5462 \\ 5454 \\ 5443 \end{array}\right\} & \text{Three faint lines.} \\[4pt]
\quad 5405 & \text{Faint line.} \\[2pt]
\quad 5167 & \text{Fine blue line.} \\[4pt]
\left.\begin{array}{l} 4481 \\ 4432 \\ 4404 \\ 4383 \end{array}\right\} & \text{Blue lines.}
\end{array}
$$

Two other blue lines, whose position could not be determined, were also seen occasionally.

II. *On the Temperature of the Flame, and the differences observed at different times.*

It is in the flame obtained on adding the highly manganiferous Spiegeleisen that the manganese-oxide spectrum is most fully developed. In the experiments at Crewe a marked difference was almost always observed between the ordinary Bessemer-spectrum and that of the Spiegel, arising from a difference in the relative intensity of the different lines. The difference was so marked that it was not at first perceived that the two spectra were in any way the same; but at Barrow this difference does not exist, the Spiegel-spectrum being identical with the Bessemer, only more intense. The ordinary Bessemer-spectrum at Barrow is in fact identical with the Spiegel-spectrum as obtained at Crewe.

A reference to fig. 2, Plate V. (which represents the two spectra on the scale of a one-prism spectroscope), will render this difference clearer.

It will be observed that in the Barrow spectrum there is a more marked division of the separate lines into groups having a general resemblance, the group η (65 to 67) being the brightest; while in the Crewe spectrum the brightest portion is nearer the red (58 and 59), and the spectrum does not extend so far into the blue. This difference is probably due simply to difference of temperature; but it can be distinctly connected with the kind of iron employed : the steel at Crewe was intended for the axles and tires of wheels; that made at Barrow was to be employed for rails. The same difference was remarked in observing two " blows " at the Bolton Iron and Steel Works. The first was of iron of No. 1 brands, and the second of inferior iron. The spectrum in the first case was the same as the Crewe one, but the Spiegel-spectrum was the same as that observed at Barrow; in the second " blow " the spectrum was nearly that of Barrow.

An attempt was made to determine the temperature of the flame by observing whether wires of gold, platinum, and of an alloy of 90 per cent. platinum and 10 per cent. iridium were melted when held in the flame. It was found that towards the middle and end of the blow the gold was always melted; but on no occasion was either the alloy or the platinum melted, even when the wire was kept in the flame for several minutes. At the beginning of the blow, however, gold does not melt. A wire held in the flame from the commencement was not melted when the sodium-line appeared flashing across the continuous spectrum, but did melt about the time that the sodium-line became constant, and before any of the Bessemer lines proper made their appearance. If we take the melting-point of gold to be 1300 C.

and of platinum 2000° C., it follows from these observations that the temperature of the flame at the commencement of the blow is below 1300°, but gradually rises, never, however, reaching 2000° C.

This result is confirmed by the fact, that of the sodium-spectrum only the double line D is present. In the Philosophical Magazine for August 1870, I showed that the sodium-lines $\left\{\begin{array}{l} 5681 \\ 5687 \end{array}\right.$ may be employed as an index of temperature, since they are present in the spectrum of any flame containing sodium the temperature of which is high enough to melt platinum, but do not appear at lower temperatures. The Bessemer-flame obviously contains abundance of sodium ; but this sodium double line is absent. The lithium orange line also, which comes out at a somewhat less temperature, is absent.

The Bessemer-spectrum is seen more or less distinctly in several other flames. The jet of flame which issues with the Spiegel from the Spiegel cupola shows it brilliantly. This flame also melts gold but not platinum. The Bessemer-spectrum is seen also in the flame from the melting-cupola for the pig iron, in the flame from the bottom of the blast-furnaces at work at Barrow, and in the flame of the coke used in warming the cupola after re-lining (which spectrum exhibits also the lines of sodium, lithium, and potassium brilliantly), and in several other flames. The flame obtained on adding the Spiegel, which gives the Bessemer-spectrum most brilliantly, is also incapable of melting platinum, but melts gold.

There are certain of the lines which seem to linger after the rest, and which, when the blow has been carried rather far, are occasionally seen alone (upon a continuous spectrum) after the regular Bessemer-spectrum has disappeared. Some, but not all of these, are iron lines. The following are those which have been noticed :—

5107, 5167, 5269 Fe, 5327 Fe, 5359, 5370 Fe, 5395, 5433, 5443, 5453, 5462.

The manganese present in the pig iron used at Barrow never exceeds 0·6 per cent. I am indebted to Mr. Richards, chemist to the Company, for the following numbers, representing the mean composition of the pig iron and spiegel iron used :—

		Pig iron.	Spiegeleisen.
Carbon {	Graphite . .	3·01	0·25
	Combined . .	0·33	4·01
	Silicon . . .	3·01	0·49
	Sulphur . .	0·06	0·04
	Phosphorus .	0·02	0·25
	Manganese .	0·42	10·15
	Copper . .	. .	trace
	Iron . .	. 93·15	84·81
		100·00	100·00

It is very difficult to understand why these lines, which are not those of carbon, should disappear at the exact point at which the blast ought to be stopped, and that they should be due to a substance of which so little can be present. The following analyses of the metal at different stages in the process, quoted from Mr. Snelus's paper read at the Meeting of the Iron and Steel Institute in December 1870, show that in some cases the quantity of manganese present is inappreciable :—

		Pig used.	6 minutes after starting.	9 minutes after starting.	Before adding Spiegel, 13 minutes after starting.
Car-bon. {	Graphite	2·07			
	Combined	1·20	2·17	1·55	0·097
	Silicon . .	1·952	0·795	0·635	0·020
	Sulphur .	0·014	trace	trace	trace
	Phosphorus	0·048	0·051	0·064	0·067
	Manganese	0·086	trace	trace	trace

XI. *On the Experimental Determination of the Relative Intensities of Sounds ; and on the Measurement of the Powers of various substances to Reflect and to Transmit Sonorous Vibrations.* By ALFRED M. MAYER, *Ph.D., Professor of Physics in the Stevens Institute of Technology, Hoboken, New Jersey, U. S. A.*[*]

WHILE the problems of the determination of the pitch of sound and the explanation of timbre have received their complete elucidation at the hands of Messenne, Young, De la Tour, König, and Helmholtz, the problem of the accurate experimental determination of the relative intensities of given sonorous vibrations has never been solved.

The method I here present will, I hope, open the way to the complete solution of this difficult and important problem ; and I trust that the success I have met with will encourage others more learned and patient to attack with superior acumen a subject which must necessarily become of fundamental importance in the future progress of acoustic research.

1. *The determination of the Relative Intensities of Sounds of the same Pitch.*

If two sonorous impulses meet in traversing an elastic medium, and at their place of meeting the molecules of the medium remain at rest, it is evident that at this place of quiescence the two impulses must have opposite phases of vibration and be of equal intensity.

I have in the following manner experimentally applied this principle to the accurate determination of the relative intensities of vibrations giving the same note and propagated from their sources of origin in spherical waves.

Clothe two contiguous rooms with a material which does not reflect sound, and place in each room one of the sounding bodies, and maintain these sounds of a constant intensity ; or the two sources of sound may be placed in the open air and separated from each other by a non-reflecting partition. Fix at a certain distance from each sounding body a resonator responding to its note; attach to each resonator the same length of firm gum tubing, and lead these tubes to a forked pipe so that the impulses from the two resonators meet at the confluence of the two branches of the forked tube, and connect the branch of the forked tube, in which the sounds meet, with one of König's manometric capsules. Now sound continuously one of the bodies, and the manometric flame, when viewed in a revolving mirror, will present its well-known serrated appearance. On sounding the second body, impulses from it will meet those from the first body ; and if the phases of vibration of the impulses on the manometric membrane are opposed and of equal intensities, the membrane will remain at rest, and the flame will now appear in the mirror as a band of light with a rectilinear upper border. But although the intensities of the pulses can easily be rendered equal by altering the distance of one of the resonators from its sounding body, yet this change of position will alter the relation of the phases reaching the membrane—so that if by mere chance we get them opposed in the first position of the resonator, they will no longer be so after its change of position. But on stopping the vibrations of one of the bodies, and setting it in vibration at intervals, we may finally succeed in causing the impulses on reaching the membrane to have opposite phases of vibration.

Such a method, which relies only on chance, can be of little value, on account of its uncertainty and the tediousness of its application.

The above difficulty I have entirely removed by the following means. I cut a piece out of one of the tubes equal in length to a half-wave of the note we are experimenting on, and replace this piece of tubing with a glass tube of the same length, into which slides, air-tight, another glass tube, also half a wave in length. The experimentation now becomes expeditious and certain. Sound both bodies continuously, and place in a fixed position one of the resonators. Move the other to a certain distance from its sounding body, and then pull out the inner glass tube until exact opposition of phase of the impulses is brought to the manometric membrane. This condition will be known when the serrations have dropped to their minimum of elevation. If the latter do not entirely disappear from the band of light in the mirror, we must place the moveable resonator at another distance and readjust the sliding tube. A few trials will give in the mirror a band of light with a straight, unruffled top border; then we have opposed phases of vibration at the confluence of the branches of the forked tube, and pulses of equal intensity are traversing the two tubes leading from the resonators.

The distance of each resonator from its sounding body is now measured; and the inverse ratio of the squares of these distances will be the ratio of the intensities of the vibrations at the sources of the sounds, if the intensities of the pulses sent through a tube from a resonator varies directly with the intensities of the vibrations of the free air in the plane of the mouth of the resonator.

It will be observed that the accuracy of the determinations by this experimental method depends on three conditions :—First, that the vibration-effects of the same area of a spherical sonorous wave diminish in intensity as the reciprocals of the squares of the distances of this area from the point of origin of the wave. There is every dynamic reason to believe in the truth of this proposition. The second necessary condition is, that the elongation of one of the resonator-tubes beyond the other by a half wave-length of firm glass tubing does not diminish the intensity of the impulses which have traversed it. Numerous experiments, especially those of Biot and Regnault on the aqueduct-tubes of Paris, show that so short a connecting-tube of glass cannot in any way affect the accuracy of the measures. The third condition is, that the intensities of pulses sent through a tube from a resonator vary directly with the intensities of the vibrations of the free air in the plane of the mouth of the resonator. This is a very important consideration; and as I believe there is no en-

tirely reliable discussion of this relation, the problem will have to be experimentally solved with the greatest care. If, however, the relation between the intensities of pulses inside the tube and those outside the mouth of the resonator shall be shown to be different (and I think they will be) from what we, for illustration, have here assumed, only the process of the numerical reduction of the experiments will be modified, while the experimental method remains secure. Indeed I cannot but consider that I have here, by applying the principle of interference, so fertile in results in optics, been the first to give an experimental method which will determine with precision the relative intensities of two sonorous vibrations producing the same note.

Savart and many other experimenters have determined the relative intensities of two sounds by placing sand or other light particles on membranes and receding from the source of sound until no motions of the particles were visible. Also Drs. Renz and Wolf (Pogg. *Ann.* vol. xcviii. p. 595) give the results of experiments on the determination with the ear of the intensity of the sounds of a ticking watch. More recently Dr. Heller (Pogg. *Ann.* vol. cxli. p. 566) has made an elaborate research on the intensities of sounds, deducing mathematically his determinations from the observed amplitudes of vibration of a membrane; and Mr. Bosanquet (Phil. Mag. Nov. 1872) has just published a paper in which he proposes, for the measure of the intensities of sounds of pipes of different pitch, the determination of the quantity of air which each pipe consumes in sounding. But all these experimenters acknowledge the want of precision in their measures, and the difficulties in the actual practice of their methods.

When the resonators have such distances from their corresponding sounding bodies that the phases of their impulses on the membrane are opposed while their intensities are different, a residual action is given; and the intensity of this action on the membrane will depend on the relative intensities of the sounding bodies and the relative distances at which the resonators are placed. It may here be interesting to consider the simplest case,—that is, when the intensities of vibration at the two sources of origin of the sounds are the same and the two resonators are placed at various distances from these points of origin, but always differ in their distances by one half wave-length. Let us call A one of the resonators, B the other. Let A be successively placed at distances from its sounding body equal to 1, 2, 3, &c. wave-lengths, and B successively at distances equal to $1\frac{1}{2}$, $2\frac{1}{2}$, $3\frac{1}{2}$, &c. wave-lengths. At each position of the resonators we will suppose that the phases of vibration reaching the membrane are opposed. The following Table gives the calculations made

on the assumption that the intensities of the vibrations diminish as the reciprocals of the squares of their distances from the sounding bodies:—

| A's dist. in λ.	B's dist. in λ.	Ratios of in-tensities.	Residual effects.
1	1·5	·444	·556
2	2·5	·640	·360
3	3·5	·734	·266
4	4·5	·790	·210
5	5·5	·826	·174
6	6·5	·854	·146
7	7·5	·871	·129
8	8·5	·885	·115
9	9·5	·897	·103
10	10·5	·907	·093
11	11·5	·914	·086
12	12·5	·921	·079
13	13·5	·927	·073
* * *	* * *	* * *	* * *
24	24·5	·959	·041
25	25·5	·961	·039

We have projected these related numbers in the accompanying curve, whose abscissæ represent the distances of A from the source of sound, and whose ordinates give the ratios of intensities between A, taken at the distances on the axis of abscissæ, and B at distances from the sounding body always one half wavelength greater than A's distance from its sounding body. The formula of the curve is

$$y = \frac{x^2}{(x + \frac{1}{2})^2}.$$

If the curve be placed upside down and referred to the corresponding numbers on the abscissæ and ordinates (which numbers are equal to unity minus the numbers at the corresponding points of the curve when in its first position), we have the graphical representation of the variation of the resultant intensities contained in the fourth column of the Table.

In the case of notes of different pitch, the higher note will necessarily force the air to make its vibrations with a greater velocity; the intensities will therefore not alone depend on the amplitudes of these vibrations, but also on their velocities; and it has been deduced from established principles of dynamics that the apparent intensities of notes of different pitch will vary directly as the squares of the amplitudes, and inversely as the fourth power of the wave-length or periodic time (see Mr. Bosanquet "On the Relation between the Energy and Apparent

3906
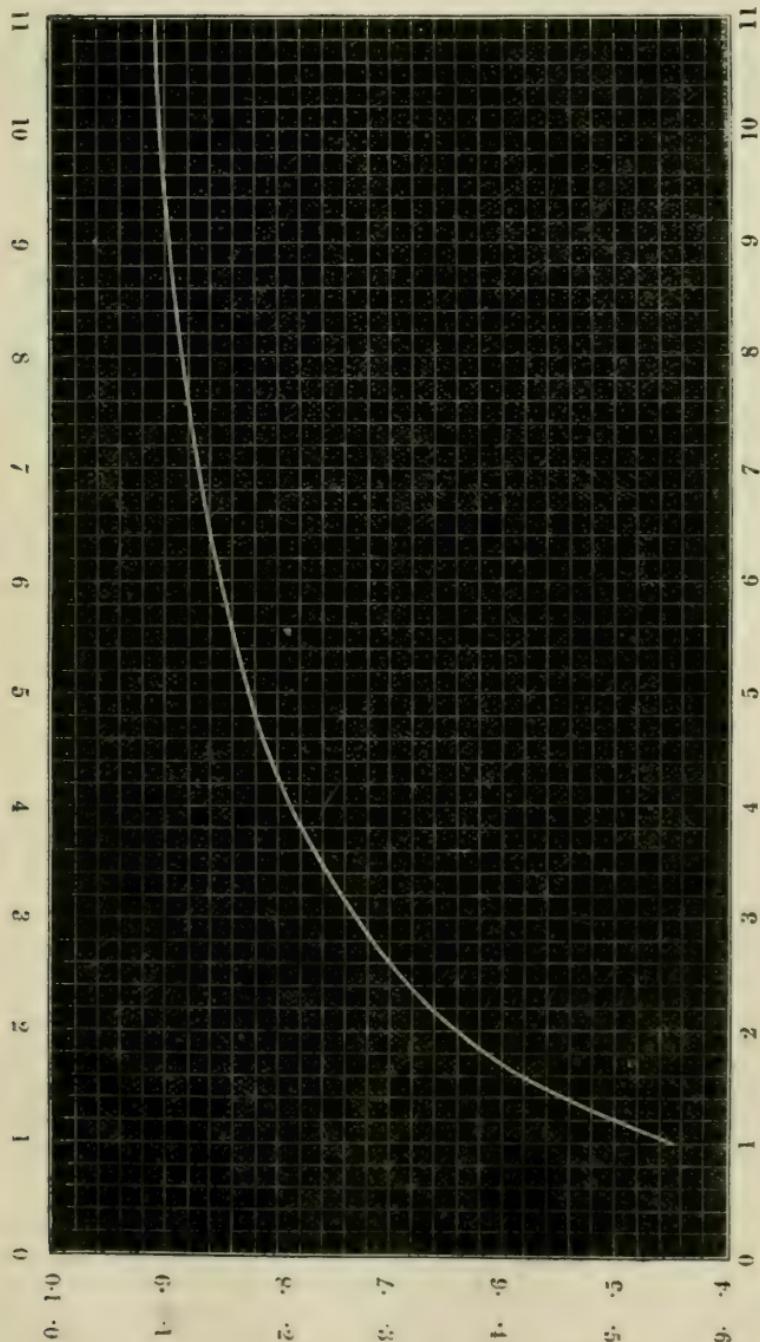
Intensity of Sounds of different Pitch," Phil. Mag. Nov. 1872).
Hence the determination of the relative intensities of notes of
different pitch becomes very complicated, and the experimental
solution of the problem is encompassed with many difficulties.
I, however, hope to be able at some future day to present some
work in this direction, when I have succeeded in obtaining results
worthy of the appellation of measures of precision.

2. *Measurement of the powers of various substances to Transmit and to Reflect sonorous vibrations*.*

After we have succeeded in obtaining a measure of the inten-
sity of the vibrations of the air at a certain distance from the

* In the Smithsonian Report for 1857 will be found an account of very
interesting and valuable experiments by Professor Joseph Henry, bearing
on "Acoustics applied to Public Buildings." In these investigations
Professor Henry determined the *relative* powers of various substances to
reflect, transmit, and absorb sonorous vibrations by placing on the bodies
the foot of a tuning-fork, and comparing the duration of its sound when
thus placed with that given when the fork was suspended in free air by a
fine cambric thread. Thus suspended the fork vibrated during 252 seconds.
Placed on a large thin pine board its vibrations lasted about ten seconds.
In this case "the shortness of duration was compensated by the greater
intensity of effect produced." The fork having been placed successively
on a marble slab, a solid brick wall, and on a wall of lath and plaster, its
vibrations lasted respectively 115 seconds, 88 seconds, and 18 seconds.

Placed on a cube of india-rubber, the sound emitted by the fork was
scarcely greater than when it was suspended from the cambric thread, but
its *duration* was only 40 seconds. Here Henry puts the question, " What
became of the impulses lost by the tuning-fork ? They were neither trans-
mitted through the india-rubber nor given off to the air in the form of
sound, but were probably expended in producing a change in the matter of
the india-rubber, or were converted into heat, or both. Though the inquiry
did not fall strictly within the line of this series of investigations, yet it
was of so interesting a character in a physical point of view to determine
whether heat was actually produced, that the following experiment was
made. . . . . The point of a compound wire formed of copper and iron was
thrust into the substance of the rubber, while the other ends of the wire
were connected with a delicate galvanometer. The needle was suffered to
come to rest, the tuning-fork was then vibrated, and its impulses trans-
mitted to the rubber. A very perceptible increase of temperature was the
result ; the needle moved through an arc of from one to two and a half de-
grees. The experiment was varied and many times repeated ; the motions
of the needle were always in the same direction, namely in that which was
produced when the point of the compound wire was heated by momentary
contact with the fingers." We have pleasure in again calling attention to
this beautiful experiment of Professor Henry ; for he was, I believe, the first
to obtain the production of heat by the absorption (so to speak) of sonorous
vibrations ; and although several experimenters have subsequently obtained
the same results, they seem to be unaware of Henry's antecedent work in
the same direction. In 1868 I published a full account of the above expe-
riment in my 'Lecture-Notes on Physics,' p. 79 (Van Nostrand, New York).

In the same paper Professor Henry obtained a few qualitative relations
on the reflecting-powers of various substances, by placing a watch between

sounding body, we can measure the powers of various substances to transmit and to reflect sonorous vibrations.

To accomplish this I place one of the sounding bodies in the focus of a parabolic reflector, and bring the two resonators to such distances from their sounding bodies that the intensities of the pulses traversing their respective tubes are equal. We then place in front of, but not too near, the mouth of the resonator in front of the reflector the plane surface of the substance whose transmitting and reflecting powers we would determine. Serrations now appear in the flame, because part of the force of the pulses which previously sounded the resonator are now reflected. The resonator which has not the reflecting surface in front of it is now gradually drawn away from its sounding body; and at each successive point of remove the pulses propagated through the resonator-tubes are brought to opposition of phase on reaching the membrane by means of the glass telescoping tube. Equality of impulses having been obtained, we measure the distance of the resonator from its sounding body; and this measure, together with the previously known distance of this resonator when equality was attained before the interposition of the reflecting surface, gives the data for the computation of the intensity of the *transmitted* vibrations. This number, subtracted from the measure of the intensity when the substance was not before the resonator, taken as unity, gives the *reflecting-power* of the substance.

It is very important in such measures to be sure that a plane-wave surface is reflected from the mirror. This character of wave can be approximately obtained by placing the mouth of a closed organ-pipe at or very near the principal focus of the mirror, and testing, by the method we have described above, the equality of intensity of the vibrating air in front of the mirror as we recede along its axis. We thus by trial at last succeed in obtaining a sufficiently plane-wave surface. Care must also be taken that the surface of the reflecting substance we experiment on is so large that no inflected vibrations can act on the resonator.

I have made several measures of Intensity and of Transmitting and Reflecting powers; but as the experiments were made in a room whose walls, ceiling, and floor gave reflected sonorous waves, I will not present measures until I have arranged suitable apartments for their accurate determination.

November 13, 1872.

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the centre and focus of a concave mirror; he then receded along the axis of the diverging sonorous beam with a hearing-trumpet. Paper and flannel were now stretched between the watch and the mirror; and the intensity of the sound was found to be diminished by the reflecting and absorbing powers of these substances.

XII. *Note on the History of certain Formulæ in Spherical Trigo-
nometry.* By I. Todhunter, *M.A., F.R.S.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

THERE are four formulæ in Spherical Trigonometry which
are usually called *Gauss's Theorems* or *Gauss's Analogies*.
These formulæ are

$$\sin \tfrac{1}{2}\, a \cos \tfrac{1}{2}(B - C) = \sin \tfrac{1}{2}A \sin \tfrac{1}{2}(b + c)$$

and three others of a like nature.

The formulæ, however, are really due to Delambre; but in
consequence of an erroneous reference, his claim has been ob-
scured, and mathematicians have been put to inconvenience in
investigating the matter.

Gauss printed the formulæ in 1809 in his *Theoria Motus,*
p. 51. He says they would be sought for in vain in books on
trigonometry; he omits the demonstration for the sake of
brevity.

Delambre, in the first volume of his *Astronomie,* published in
1814, claims the formulæ (see his pages 164 and 195); in both
places he refers to the *Connaissance des Tems* for 1808. The re-
ference, however, should be to the *Connaissance des Tems* for
1809, which was published in April 1807. Here the four for-
mulæ are given without demonstration, together with some
others which follow immediately from them. One of these
other formulæ is

$$\tan \tfrac{1}{2}\, a = \frac{\tan \tfrac{1}{2}(b - c) \sin \tfrac{1}{2}(B + C)}{\sin \tfrac{1}{2}(B - C)} ;$$

this Delambre ascribes to M. Henri.

As Delambre is here discussing the solution of an astrono-
mical problem given by M. Henri, it might at first sight have
appeared probable that *after* this formula had been used by M.
Henri, the four formulæ improperly ascribed to Gauss were in-
vestigated by Delambre. But it may be inferred from the top
of page 446 of Delambre's remarks, that he had been previously
acquainted with the formulæ.

Delambre claims the formulæ and gives the correct reference
on p. 349 of the *Connaissance des Tems* for 1812, which was
published in July 1810: Delambre is here reviewing Gauss's
Theoria Motus.

Delambre's erroneous reference has been adopted by some
writers. Thus Bowditch says on p. 737 of the first volume of
his translation of the *Mécanique Céleste,* " Delambre, in his *As-
tronomie,* vol. i. p. 164, observes that he had given several of

these theorems in the *Connoisance des Tems*, 1808, before the publication of the work of Gauss. . . ." T. S. Davies says, on p. 37 of the second volume of the twelfth edition of Hutton's 'Course of Mathematics,' " The four formulæ . . . are usually known as *Gauss's Analogies*, their demonstration having been first given by that illustrious geometer in his *Theoria Motus Corporum Cœlestium* (1809): but they had been published by Delambre some years previously in the *Connaissance des Temps* (for 1808) . . ." Here, besides the expansion of an exact *two* years into an indefinite *some* years, we have the statement that Gauss gave a *demonstration* of the formulæ in 1809 ; but as we have already stated, Gauss omitted the demonstration. In a note Mr. Davies adds : " Gauss did not deliver his theorems, or their investigations, in precisely the forms given in the text . . ." But Gauss *did* deliver his theorems in those forms. Then what Mr. Davies goes on to say respecting the forms and investigations may perhaps apply to some other work, but does not apply to the *Theoria Motus*, where Gauss delivered the theorems.

It must be observed that Gauss had been anticipated even in Germany in the publication of the formulæ. They were given by Mollweide in Zach's *Monatliche Correspondenz* for November 1808, with a demonstration.

The subject is noticed in an article in Klügel's *Mathematisches Wörterbuch*, vol. v. p. 211 ; the passage has been reproduced in the ' Proceedings of the London Mathematical Society,' vol. iii. p. 320. The writer states correctly the positions of Gauss and Mollweide ; and then he adds that Delambre published the formulæ in the *Connaissance des Tems* for 1808, and so French writers usually call them after him. But these few words relating to Delambre seem to me to fall below the usual high standard of German accuracy. For in the first place the erroneous date (1808) must have been borrowed without verification, although there is nothing to warn us of this. And in the next place the writer apparently puts the claims of Mollweide and Delambre as equal, by ascribing to both the date 1808, overlooking the fact that the *Connaissance des Tems* for an assigned year is published in advance of that year.

Thus, finally, although Mollweide has priority over Gauss, yet he comes about a year and a half after Delambre ; and therefore until any other person can be shown to have published the formulæ before April 1807, they must be justly ascribed to Delambre.

Demonstrations of the formulæ in two ways were published by Delambre in his *Astronomie* (see pp. 164 and 196 of his first volume). It would appear from his page 164 that he considered this to be the first publication of a demonstration ; but,

as we have stated, Mollweide gave a demonstration in 1808. In his second way, Delambre makes use of a diagram from which he obtains both his own Analogies and those of Napier. This way of demonstration is substantially the same as that which was independently discovered and printed in the 'Proceedings of the London Mathematical Society,' vol. iii. p. 13. One step in the recent process, however, is simpler than the corresponding step in Delambre's, namely the proof of the equality of the angles M V A and C V P.

It may be remarked that if one of Napier's Analogies is given, we may deduce another immediately by using one of the triangles *associated* with the fundamental triangle; and then Napier's two other Analogies follow by the aid of the polar triangle. Thus we may say that the other three may be deduced immediately from any one of them. But with respect to Delambre's Analogies, the case is rather different. Take these in the order in which they are given in my 'Spherical Trigonometry.' Then (1) and (4) are so related that either can be deduced from the other by using an associated triangle; but nothing new is obtained from (1) or from (4) by using the polar triangle. And (2) and (3) are so related that either can be deduced from the other by using an associated triangle, *or* by using the polar triangle. Thus from one of Delambre's Analogies we cannot deduce immediately the other three. If one of Napier's Analogies is given and one of Delambre's, we can deduce immediately the other six. Also the other six may be deduced immediately from (1) and (2) of Delambre's Analogies; and the other six may be deduced immediately from (3) and (4) of Delambre's Analogies.

<div style="text-align: right">I. Todhunter.</div>

Bourne House, Cambridge.

XIII. *On the Law of Gaseous Pressure.* By Robert Moon M.A., *Honorary Fellow of Queen's College, Cambridge* [*].

I DESIRE to offer some remarks upon Mr. Strutt's further criticism[†] of my views as to gaseous pressure, for which I have not had opportunity hitherto.

I fail to find in Mr. Strutt's second paper any reply to my inquiry why we are to reject the formulæ

$$p = -\frac{a^2}{\rho} + \phi\left(v + \frac{\alpha}{\rho}\right), \quad . \quad . \quad . \quad . \quad (1)$$

$$v + \frac{\alpha}{\rho} = \psi_1 \left\{ x - \frac{\phi'(u) - \alpha}{\rho} \cdot t \right\},$$

$$\frac{1}{\rho} + \int \frac{du}{\phi'(u) - 2\alpha} = \psi_2 \left\{ x - \frac{\alpha}{D} \cdot t \right\}$$

(where $u = v + \frac{\alpha}{\rho}$), which, as Mr. Strutt appears to be fully conscious, satisfy the equation of motion

$$0 = \frac{d^2y}{dt^2} + \frac{1}{D} \frac{dp}{dx} *. \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (2)$$

In his first paper (see Phil. Mag. for July last) Mr. Strutt appeared to regard Boyle's law as experimentally established in all cases of rest or motion—a circumstance which, if it were true, would be decisive as to any value which the above formulæ may possess in physics. Inasmuch, however, as after my pointing out that the proof of Boyle's law was limited to the case of equilibrium, Mr. Strutt does not repeat his statement, I might conclude that he had altered his opinion, but for the occurrence of certain expressions of a contrary tendency.

I desire to know definitely, therefore, first, whether Mr. Strutt still considers that Boyle's law has been experimentally proved in the case of motion, and, secondly, what are the experiments upon which he rests this conclusion.

From not having bestowed sufficient consideration on the formulæ, Mr. Strutt has completely misapprehended their significance.

For, suppose that, when $t = 0$, ρ and v have respectively the definite values $f_1(x)$, $f_2(x)$, then (1) gives us

$$p = -\frac{\alpha^2}{f_1(x)} + \phi \left\{ f_2(x) + \frac{\alpha}{f_1(x)} \right\}; \quad \cdot \quad \cdot \quad (3)$$

but this does not determine the law of pressure which prevails when $t = 0$, for the obvious reason that ϕ is arbitrary. So far is it from being the fact that a knowledge of the law of density and the law of velocity prevailing in the fluid at a given time

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* Mr. Strutt, after writing the equation

$$\frac{dv}{dt} = \frac{1}{D} \frac{dp}{dx},$$

properly remarks that another equation requires to be added, viz.

$$\frac{d}{dt} \left(\frac{D}{\rho} \right) = \frac{dv}{dx}.$$

This last, however, is included in the single equation of the text, in virtue of the analytical condition, $\frac{d}{dt} \left(\frac{dy}{dx} \right) = \frac{d}{dx} \left(\frac{dy}{dt} \right)$, and of the fact that $\frac{dy}{dx} = \frac{D}{\rho}$.

enables us to determine the law of pressure in the fluid at that time, that the law of pressure at the time in question may be any whatever. In fact, if when $t=0$ we have $p=f_3(x)$, where f_3 denotes any continuous function, we shall have

$$f_3(x) = -\frac{\alpha^2}{f_1(x)} + \phi\left\{ f_2(x) + \frac{\alpha}{f_1(x)} \right\}, \quad . \quad . \quad (4)$$

an equation which can be satisfied by means of ϕ, and of which indeed it is the special and exclusive office to determine ϕ. This can be done as follows :—

Putting
$$f_2(x) + \frac{\alpha}{f_1(x)} = \omega,$$

and solving with respect to x, we get

$$x = \text{funct. } \omega = f_a(\omega) \text{ suppose.}$$

Hence (4) becomes

$$f_3\left\{ f_a(\omega) \right\} = -\frac{\alpha^2}{f_1\left\{ f_a(\omega) \right\}} + \phi(\omega),$$

which determines the form of ϕ.

It thus appears that the office performed by (1) is of this kind ; viz. the law of pressure prevailing at a certain time, and also the laws of velocity and density prevailing at the same time being given, (1) enables us from those data to determine the law of pressure prevailing in the same case of motion at any other time. Obviously, therefore, it is as unreasonable for Mr. Strutt to ask me to state "the real physical law of pressure true at all times and places," as it would be for me to require him to determine in any particular case of motion the velocity and density at any instant by means of Euler's equations, without affording him any information as to the circumstances of the initial motion.

Of the particular cases of failure which I have adduced against the received law of gaseous pressure, the first which Mr. Strutt considers is that of a closed cylinder filled with air, which at the time t is destitute of velocity, but in which the density to the right of a certain plane is uniformly equal to 2D, while that to the left of the plane is D. I contend that if the received law of pressure held under these circumstances, it would contradict the principle that action and reaction are equal and opposite.

Mr. Strutt meets this by suggesting that "an infinitely small . . . . layer of air situated at the boundary is subject to an infinite acceleration," and that the fact "that the pressures which act on its two faces are unequal is therefore not in contradiction to any true principle."

Now by "infinitely small" Mr. Strutt must mean here "indefinitely small," whereas all the circumstances which I have

supposed to exist at the time *t* are precise and definite. That any thing indefinite should arise out of that state of circumstances is simply impossible. In the case supposed, acceleration may, and indeed will occur at the time *t*; but the breadth, great or small, over which it prevails must be a *definite* breadth; and whether great or small, if it have magnitude, if it have existence, throughout that breadth Boyle's law does not hold. Mr. Strutt's apology for the law, therefore, in this case of its manifest failure, is, in effect, simply an admission of its failure, coupled with an wholly unfounded assertion that the failure will be confined within extremely narrow limits.

The next case treated of by Mr. Strutt is where a vertical cylinder closed at its lower end has an air-tight piston, capable of working freely in the upper part of it, which is exactly supported by the air beneath. I contend that if the received law of pressure were true, the placing of an additional weight at the time *t* upon the piston under the above circumstances would not destroy the equilibrium; for at the time *t*, when the weight is upon the piston, the density is unaltered; therefore, according to the received law the pressure of the air upon the piston will be unaltered, and the pressure of the piston upon the air (which must be exactly equal to the latter) will also be unaltered; *i. e.* the introduction of the additional weight leaves the actions between the different parts of the system precisely what they were during equilibrium*.

Mr. Strutt, on the other hand, maintains that "precisely the same argument may be used to prove that a body cannot begin to fall under the influence of gravity; for a body cannot leave its initial position without acquiring velocity, and (by the law of energy) cannot possess a velocity without having already fallen.

Now the fallacy of this argument, to prove that a body cannot fall from rest under the influence of gravity, may be exposed in a moment. If by the allegation that "a body cannot leave its initial position without acquiring velocity" is meant that acquisition of velocity is the necessary result of its leaving the initial position, the argument involves a *non sequitur*. For, granting what is here affirmed, and granting, with or without the aid of the law of energy, that the body "cannot possess a velocity with-

* Another way of expressing the argument is as follows:—Change of density can only occur through motion of the particles; but the particles being originally at rest and the system in equilibrium, motion of the particles can only occur from change of pressure. In other words, change of pressure (the cause) must precede the motion which it effects, and must precede therefore the change of density which results from the latter. But the received law of pressure asserts the contrary, viz. that change of density must precede change of pressure, which is absurd.

out having fallen," the conclusion arrived at simply does not follow from the premises.

If, on the other hand, by the above allegation is meant that a body cannot move from a particular position unless it have previously acquired a certain velocity, the point to be proved is simply taken for granted.

If I have misunderstood the point of Mr. Strutt's paradox, I trust that he will set me right in regard to it; and as he must be well acquainted with the fallacy which it involves, he will perhaps not object to state distinctly in what that fallacy consists.

As to the paradox which I have brought forward, I contend that its fallacy consists in the false assumption of the received law of pressure. If Mr. Strutt is not satisfied with this explanation, I must call upon him to state definitely the particular point in which my reasoning is defective, instead of contenting himself with a vague assertion of analogy, where, as I contend, nothing of the kind exists.

Mr. Strutt thinks I " must admit that it is remarkable that so apparently reasonable a law should lead to such absurd conclusions."

I view the matter in a totally different light. In any systematic investigation of the subject which may be made in the present state of our experimental knowledge, it appears to me so much a matter of course to assume that the expression for the pressure will contain both velocity and density, and the contrary supposition appears to me to be so opposed to every sound principle, that the signal failure of the latter is exactly what I should have expected. It was in fact this anticipation that led me to the examination which has resulted in the detection of the cases of failure I have here and elsewhere adduced. With the statement of one of them, of somewhat peculiar character, I will conclude these remarks.

In the last case, a vacuum being supposed to exist above the piston, suppose that instead of an additional weight being introduced the piston is suddenly removed at the time t, then at the time t, the density of the air being throughout unaltered, we shall have according to the received law a finite pressure at the highest point of the aërial mass; i. e. *we shall have a pressure where nothing is pressed*!—a conclusion opposed alike to the dictates of common sense and the significance of language.

6 New Square, Lincoln's Inn,
    January 6, 1873.

XIV. *On Manometric Flames.*
By Dr. Rudolph König (*of Paris*).

[Concluded from p. 18.]

[With a Plate.]

Decomposition of Sounds into their simple Tones.

THE same resonators (Helmholtz's) which serve for analysis of sounds by means of the ear, are also of use for the visible dissection of sounds by the flames. To this end I construct an apparatus with eight resonators tuned to the harmonic notes of *c*, each of which is connected with a manometric flame. These eight flames are placed in a slanting line one above the other, and show, in the rotating mirror fixed in the same direction, eight parallel bands of light when in repose, and when in vibration eight waved lines (fig. 11), Of course in this case each

Fig. 11.
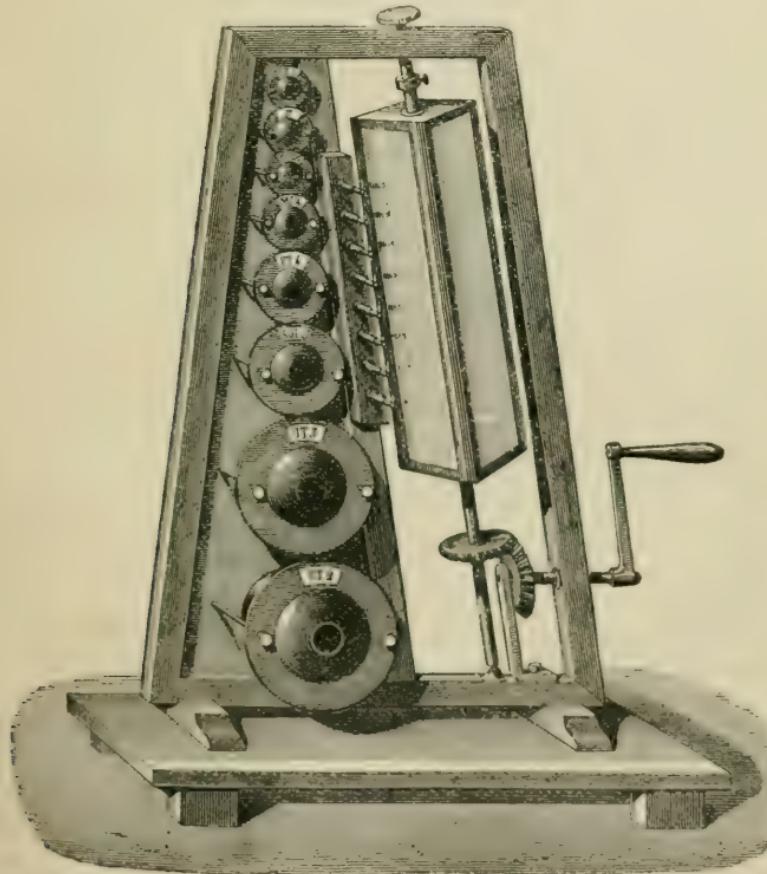
flame must be perfectly independent of the other, and each flame

vibrate only when its particular resonator is put in action by a
note in unison; the notes *not* contained in the series of reso-
nators must have no effect whatever on any of the flames.   In
order to show how far the apparatus fulfils these conditions, I
usually employ a series of tuning-forks on sounding-chests,
which, particularly a few moments after being sounded, give
almost simple notes.

I first take forks which are in tune with the resonators, and
sound them singly, and show that only the bands of light which
correspond to their notes dissolve into vibrations, so that several
simple notes must be sounded to cause the appearance of several
serrated bands of light.   By means of a tuning-fork not in tune
with the resonators, I can then show that its note, even when
sounded with considerable force, has no effect on the flames.   A
note sounded with very great intensity may indeed have some effect
on all the flames, through the resonators; but this case will not
give rise to error, as all the flame-series appear equal, whereas,
when resonance takes place, the number of the single flame-
waves in the series increases upwards in the proportion of $1 : 2 : 3$
&c., and their width, of course, decreases in the inverse ratio.

After demonstrating in this way the nature of the apparatus,
I produce before it a sound whose fundamental is *c*; and the ser-
rated bands of light then show by what harmonic notes the fun-
damental is accompanied, as well as the relative intensities of
these notes.   If before the apparatus we play the *g* of the violin,
for which the apparatus has no resonator, the octave $\overline{g}$ vibrates
strongly, and the $\overline{\overline{c}}$ of the same instrument resolves, together
with the flame of the fundamental, that of the octave *c*.   An
open organ-pipe, of small diameter, tuned to *c*, when forcibly
blown, set the first five flames in vibration, but the third vibrated
more strongly than the octave.   A closed organ-pipe with the
same fundamental caused the twelfth to appear very strong, and
the fifth very weak.   A protruding tongue without a sounding-
cup resolved the first six harmonic notes with pretty regularly
decreasing intensity.

On singing the vowel U, the octave as well as the fundamental
shows rather strong vibrations, and only sometimes a trifling
effect may be observed on the third note.   D, on the contrary,
excites the flames of the third and fourth notes very strongly,
while the vibrations of the octave are weaker than with U.   The
fifth flame-band is serrated, but slightly, with O.   With O A
the region of greatest intensity becomes higher; it is the fourth
and fifth notes which show the deepest indentations in the band
of light, while the lower notes are weaker.   With A all the flames
are resolved up to the seventh, and the fourth, fifth, and sixth

vibrate with great force. When E is sung, we see the fundamental accompanied by the octave weakly, and very strongly by the twelfth. The double octave and its third show vibrations of moderate intensity; and the seventh flame shows traces of the existence of the seventh tone. The letter I sung on *c* gives a strong movement to the flames of the octave and the fundamental only, while all the other flames are in repose.

The resonators 7 and 8 ($\overline{\overline{c}}$) of the apparatus cause their flames to vibrate with difficulty, and the notes must be very strong. We have now reached the limits within which the flames can be usefully employed.

As this apparatus does not permit us to choose the fundamental tone of the vowel or of any other sound which is to be analyzed, it is adapted rather to demonstration than to further investigation. However, to make it more useful for the latter purpose, I have constructed a second model (fig. 12), in which the

Fig. 12.
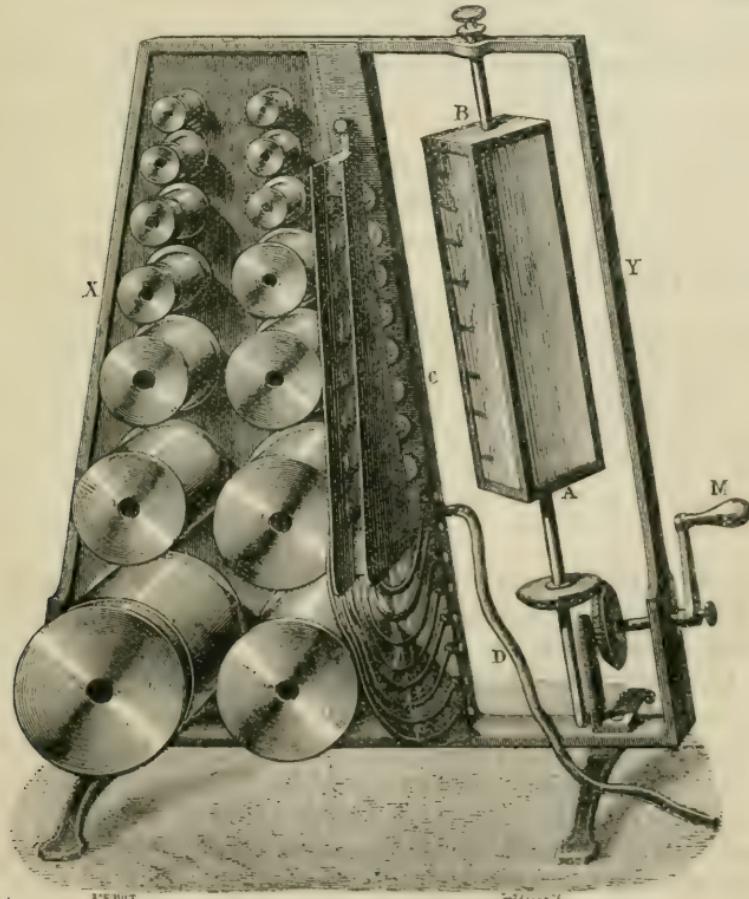
eight spherical resonators are replaced by fourteen universal resonators. These resonators consist each of a cylinder, its length about equal to its diameter, which is formed by two pipes placed one within the other. The outer of these pipes terminates at one end in a hemisphere, from which the tube for the ear is carried, as in the spherical resonators. The opposite end of the inner pipe is closed by a plate, in the middle of which there is an opening for the passage of the enclosed air to the exterior atmosphere. This arrangement permits us by drawing out the pipe to increase the mass of air in the resonator, and to lower its tone by a third. On the inner tube lines are drawn which indicate how far the outer one must be drawn out for the different notes. The deeper resonators of the series are so constructed that the highest note of the larger shall always reach to the lowest of the next smaller one. In the higher-toned resonators this would not be sufficient, because the sixth, seventh, and eighth accessory notes approach each other so nearly that the necessity might occur of forming two of them with the same resonator. Since, therefore, the highest notes of the deeper are a whole note above the lowest notes of the next upper resonator, the whole series contains the following notes:—1, G–B; 2, B–*dis*; 3, *dis–fis*; 4, *fis–a*; 5, *a–c̄*; 6, *c̄–ē*; 7, *ē–gīs*; 8, *gīs–c̄*; 9, *c̄–ē*; 10, *d̄–f̄*; 11, *ē–gīs*; 12, *f̄–ā*; 13, *gīs–ē*; 14, *c̄–d̄*.

The series of overtones for the notes of both octaves from C–c̄ are to be found in the resonators placed opposite to each in the following Table :—

C :	2,	4,	5,	6,	7,	8,	9,	10.	c :	2, 5,	7,	8,	9,	11,	13,	14.
D :	2,	4,	6,	7,	8,	9,	10,	11.	d :	2, 6,	8,	9,	10,	12,	13,	14.
E :	3,	5,	6,	7,	8,	9,	10,	11.	e :	3, 6,	8,	9,	11,	13,	14.	
F :	3,	5,	7,	8,	9,	10,	11,	13.	f	3, 7,	8,	11,	12,	13.		
G :	1,	4,	6,	7,	8,	9,	10,	11,	g :	4, 7,	9,	11,	13.			
A :	1,	4,	6,	8,	9,	10,	11,	12,	a :	5, 8,	9,	12,	14.			
B :	1,	5,	7,	8,	9,	11,	12.	13,	b :	5, 8,	11,	12.				
									c̄ :	5, 8,	11,	13.				

For the fundamentals C–F the resonators are wanting, but one can make observations up to the ninth note of the sound. For the sounds G–d the resonators serve to the eighth note; then they begin to fail; at *e* we can employ only six flames, at *f* five; and at last at *c̄* but three for the overtones.

Although, as before mentioned, it is indicated on each resonator how far it must be drawn out for the different notes, yet it is as well, in order to have exact results with the apparatus, particularly if the fundamental of the sound to be investigated does not exactly coincide with one of the indicated notes, to employ

the following mode of giving the desired pitch to the resonators in question.

Tune a string of the sonometer to the fundamental tone of the sound, and produce on it the harmonic notes one after another. Then place the proper resonator in communication with the ear instead of with the manometric capsule; and while the india-rubber tube is in the ear, it becomes very easy to determine their arrangement and the position for the strongest resonance.

After having tuned eight of these resonators to c and its over-tones, I repeated the same experiments with this apparatus as I tried on the spherical-resonator apparatus, and obtained exactly the same results. There was not the least sign of any weakened sensitiveness in the flames; so that this apparatus appears to me exactly fitted for more exact and searching experiments on sounds in general, and particularly those of the human voice, at least those composed of notes which do extend beyond $\overline{\overline{c}}$. It is to be remarked that direct employment of the resonators with the ear does not succeed far beyond this limit.

Unfortunately I am now convinced that the state of my voice does not permit me to investigate any further in this direction, as I had intended; so I must be content to show the capabilities of the apparatus, as I shall again, when describing the method of experimenting on the vowel-sounds, and others also, by the elimination of single accessory notes, or whole series of them.

Interference-phenomena.

In my description of the results obtained by the combination of the notes of two organ-pipes I have not mentioned *unison*. The combination of two notes in unison has a special interest, on account of the communication of the vibrations and the interference-phenomena which may be observed therein. I therefore preferred deferring their description until now, when I could explain them in connexion with other similar experiments.

If we place two organ-pipes tuned in unison in communication with two flames and sound only one of them, the flame of the other shows that its air-column also vibrates in sympathy through communication; and this passing on of the vibrations takes place even if the organ-pipes are not in exact unison with each other, and therefore when sounded together cause *beats* to be heard. But it is to be remarked that in this case the sympathetic pipe does not form its own vibrations, but only vibrations which are exactly in unison with those of the one acting on it, so that beats are neither heard nor their effects seen in the flame. If, however, we blow the second organ-pipe and thus cause its own vibrations, they unite with the resonance-vibrations, and the flame shows clearly, by

its violent flickering, the existence of beats, which are also heard distinctly.

I draw particular attention to this isolated occurrence of the resonance-vibrations in the air-column, because it is not exhibited by the influence-phenomena of two strings stretched above the same sounding-board; but the proper vibrations combined with resonance-vibrations appear in the string influenced, without its being struck or bowed.

It is known that the beats of two such mutually sympathetic strings accommodate themselves to each other in such a manner that the one reaches the maximum amplitude of its vibrations when the other is at the minimum. Now the flames of the two sympathetic organ-pipes exhibit the same phenomenon, for as the one rises the other falls; both, however, must be blown at the same time, whilst it is only necessary to play on one of the strings.

When the pipes are in perfect unison, and their single vibrations mutually adapt themselves in the same way as the beats did, *i. e.* that in the node of the one there is a condensation of the atmosphere when in the other a rarefaction takes place, then the whole process can be clearly observed in the two flames if we place them one beneath the other in a vertical line. Both flames show their vibrations unweakened; yet their individual pictures in the rotating mirror are not beneath each other in the two lines, but alternate.

If both notes act together on the same flame, they, of course, at the beats show more violent flickerings than did the two flames; for the latter were produced by direct and by sympathetic and therefore unequally strong vibrations in the same air-column, whilst the present ones are formed by direct and therefore nearly equally strong notes in two similar air-columns. If the two notes are approximated gradually to unison, we observe that the oscillations cannot be made slower at will, as with tuning-forks, but at a certain limit they disappear suddenly, and both air-columns vibrate as one system, *i. e.* as two somewhat differently tuned bodies that are so closely united and therefore act so strongly on one another that neither can give its proper note in its integrity, and the consequence is that only a single intermediate note is produced. This note is more powerful than that of a single organ-pipe; and the flame shows in the centre of its interior a brilliant waist, which rises above a non-brilliant blue broad hollow space. As it approaches perfect unison more and more, the height of this dark space increases, the brilliant waist vanishes; and when unison is attained the flame appears in complete repose. At the same moment the strong fundamental has almost disappeared, and we hear the first overtone clearly

produced ; for it is known that, there being a difference of half a vibration-period between two equal sounds in unison, while the fundamental and the odd overtones are destroyed, all the even overtones in both sounds vibrate without difference of phase and strengthen one another. The flame also makes the octave recognizable in the rotating mirror, since we see a series of low wide flame-pictures, of which each single one is forked. It is well in this experiment to employ a rather stronger air-pressure, in order to increase the intensity of the octave in the sound of the pipes.

As this prominence of the octave at the interference of the fundamentals of two sounds is demonstrated particularly well by means of the double siren of Helmholtz, I represented the phenomenon in this case also by the flames. To this end I provided each of the two sounding-chests over the turning plates with a tube, which permitted its interior space to be placed in direct communication with the tube leading to the capsule. This tube was of india-rubber, thus retaining the power of movement within certain limits for the upper wind-chest of the siren, so as to be able by its different positions to produce the interference or to withdraw it. Invariably, if we approach the siren-chest to the interference-place, we see the great vibrations of the fundamental gradually disappear, and the short forked flame take their place as the picture of the octave.

A particular apparatus, which I construct for the observation of interference-phenomena of the most various kinds, is founded on the method first employed by Herschel, and after him by many natural philosophers. This is to produce interference by permitting the waves from the same source to traverse two courses differing in length by half a wave, and then to reunite them. It consists of a tube that between its ends branches into two arms, one of which can by drawing out be lengthened at will (fig. 13). If we wish a complete interference, we must introduce a simple note into the tube, which is joined to a resonator, before which we sound the proper tuning-fork. If we now lengthen the one arm until the difference of length of the two is equal to half the wave-length of the note of the tuning-fork, the waves coming from the two arms are mutually destroyed at the other end of the tube ; and if we fix this into a small cavity, over which a manometric capsule is placed, we see, on drawing out one of the arms of the tube, how the at first deeply serrated flame-series in the rotating mirror gradually transforms itself into a simple band of light, until the difference of a half wave-length is attained.

But the interference can be shown still more beautifully by another arrangement. Instead of causing the arms united to a

single tube to act on a capsule, I place a small apparatus to both
exits of the two tube-branches; this is so arranged that now

Fig. 13.
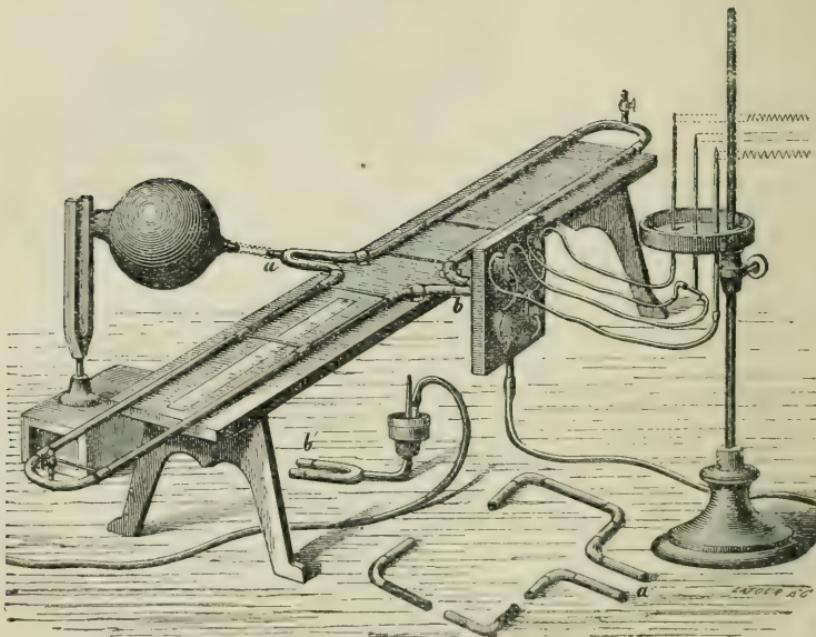
each branch is in communication with a separate capsule. These
two capsules, whose action on each other is annulled by two ac-
cessory capsules, are provided with two gas-pipes instead of one.
On a stand are placed three burners, which are fixed at different
elevations; the centre one is arranged for the reception of two
india-rubber tubes.   I connect now one gas-pipe of the one cap-
sule with the highest burner, one pipe of the other capsule with
the lowest, and by means of the remaining two exit-pipes I place
both capsules in communication with the centre burner.   If I
now strike the tuning-forks while the lengths of the tube-branches
are equal, the three flames in the rotating mirror show three
equally deeply serrated flame-series one above another, of which
the centre alone changes into a simple band of light on length-
ening one of the branches a half wave-length of the note, while
both the other flames continue to vibrate with unchanged inten-
sity.   Thus we have here at the same time a view of the action
of the sound-waves when they approach through the one arm
alone, when they have passed through the second only, and
also when they arrive united at the flame after passing through
both.

    If in these experiments we employ instead of a tuning-fork

with a resonator an open organ-pipe of not too great diameter, during the interference of the waves of the fundamental the vibrations of the octave become again prominent. By interference we can remove not only the fundamental, but any overtone we please from a sound, as may be clearly demonstrated with the above-described covered pipe. I conduct the sound into the apparatus, while I connect with it, after the removal of the gas-burner, the capsule at the end by means of an india-rubber tube. If I then draw out the one tube so far that interference ensues for note 3, the centre flame in the mirror shows the simple flame-series of the fundamental, while the two others form the picture before described (fig. 5, Pl. I.), resulting from the combination of notes 1 and 3. In the same way we can banish from vowel-sounds various overtones, or rather whole series of them, which offers a new and fruitful method for the investigation. In these experiments the arrangement with three flames is particularly useful, because the upper and lower flames remaining always unchanged permits the slightest alteration in the middle one to be observed. Thus, for example, the vowel U sung on $\overline{c}$ into the apparatus shows the fundamental only weakly, accompanied by the octave. If we place the apparatus so that

the waves of $\overline{\overline{c}}$ interfere, every trace of this octave is lost, whereas on the interference of the fundamental two narrow flames of almost equal height take the place of each wide flame; these narrow flames represent the octave, now almost alone. With O sung on the same note (when the fundamental is accompanied much more strongly by the octave than with U) we can make the same experiments; only here at the interference of the octave the note 3 becomes prominent, whilst the wide flame of the fundamental spreads out into three diminishing summits. A sung on $\overline{c}$, at the interference of the third note brings forward strongly the octave with the fundamental. If the waves of the octave interfere, there appears a group of five flame-summits, which appear to indicate the notes 1, 3, and 5. If we suppress the fundamental and with it the notes 3, 5, &c., there appears a simple flame-series, which is formed by the octave alone.

These phenomena are nevertheless not always of so simple a nature as in these examples, when it is a question of more composite flame-groups of the deeper sounds; and therefore I will now call attention to the fact that, on lengthening one of the tubes of the apparatus, we often see suddenly very great changes in the flame-picture when the former is between the interference-points of two successive overtones of the sound. This is then the interference-point of the lower octave, or twelfth of a higher overtone of the sound, which is in this way removed.

In the place of the forked tube into which in all the foregoing experiments the note or sound was introduced, we can put two separate tubes of exactly equal length and form, each consisting of three separate pieces inserted one in another and capable of being turned round so that we can move the two openings at their ends in any direction we please without alteration of the length of the tube or of the form of its turnings. This arrangement permits then the entrance of the note of two different points of a vibrating body into the apparatus—for example, of two vibrating bridges of a plate with contrary signs, or of the same place on its opposite surfaces : in both these cases the interference takes place when the two paths are equal, and the tone first becomes audible when the interference is destroyed by lengthening one of the compound tubes.

In order to adapt the apparatus to the demonstration of the wave-lengths of a note in different gases, and for the experiments of Zoch, I have provided the pipes with two cocks, which serve to fill and empty them.   Of course, if we experiment with any other gas than atmospheric air, the resonator cannot remain in direct communication with the interior of the pipe ; and therefore we must in that case place between them a small cavity, which is divided in the centre by a thin membrane into two halves—the one to be united with the pipe, the other with the resonator. Besides we must then have india-rubber rings to draw over the ends of the tubes which are only placed within each other, so that the gas cannot escape at these places.

It is, of course, understood that this apparatus permits the direct observation of different interference-phenomena by the ear, and consequently the repetition of the experiments of Mach, Quincke, and others.   For this purpose we have but to place one of the forked tubes before the apparatus and connect the former with the ear by an india-rubber tube.

XV. *On the best Arrangement of* Wheatstone's *Bridge for measuring a given resistance with a given Galvanometer and Battery.* *By* Oliver Heaviside, *Great Northern Telegraph Company, Newcastle-on-Tyne*[*].

IN the figure, a, b, c, and d are the four sides of the electrical arrangement known as Wheatstone's bridge or balance, e the galvanometer, and f the battery branch.   Throughout this paper d is supposed to be the resistance to be measured, and e and f both known.   The problem is to find what resistances should be given to the sides a, b, and c (which we are able to

vary), so that the galvanometer may be affected the most by any slight departure from the balance which occurs when $a : b = c : d$.
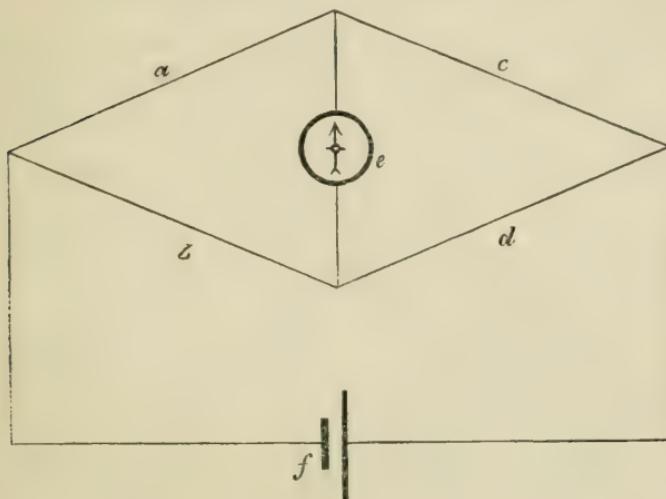
The nature of this problem may be more easily understood from the following considerations :—

1. If b, c, d, e, and f are given, then there is only *one* value of a which will produce a balance, viz. $a = \dfrac{bc}{d}$.

2. But if c, d, e, and f are given, but not b, then there is an infinite number of pairs of values of a and b which will produce a balance by satisfying the relation $a : b = c : d$; and one particular pair will constitute the best arrangement, by which is meant that the galvanometer will be most sensitive to any slight departure from the equality of $\dfrac{a}{b}$ and $\dfrac{c}{d}$ when those particular values of a and b are used.

3. And if only d, e, and f are given, then for any value we give to c there is a pair of values of a and b which constitutes the best arrangement for that value of c; and there will be a particular value of c which, with the corresponding values of a and b, will be the best arrangement for the given values of d, e, and f.

In order to find what functions a, b, and c must be of d, e, and f to constitute the best arrangement, it will be first necessary to find the best values of a and b when c, d, e, and f are given. This I now proceed to do.

It is well known, and may be easily proved by Kirchhoff's laws, that the current passing through the galvanometer is re-

presented by $u = \mathrm{E} \times$

$$\frac{(a+b+c+d)(ad-bc)}{\{(a+b)(c+d) + (a+b+c+d)e\}\{(a+c)(b+d) + (a+b+c+d)f\}}, \quad (1)$$

in which E is the electromotive force of the battery. $(ad-bc)$ may be positive, negative, or nothing, in which last case $u = 0$, and a balance is obtained, no current passing through the galvanometer.

Dividing both numerator and denominator of (1) by

$$(a+b+c+d)^2,$$

it becomes

$$u = \mathrm{E} \times \frac{\dfrac{ad-bc}{a+b+c+d}}{\left\{\dfrac{(a+b)(c+d)}{a+b+c+d} + e\right\}\left\{\dfrac{(a+c)(b+d)}{a+b+c+d} + f\right\}}; \quad (2)$$

from the form of which it may easily be seen that the best value of the resistance of the galvanometer e, when a balance is obtained and the other resistances are fixed, is, as Schwendler has shown in the Philosophical Magazine for May 1866,

$$e = \frac{(a+b)(c+d)}{(a+b+c+d)} = b \cdot \frac{c+d}{b+d}; \quad \cdot \quad \cdot \quad \cdot \quad (3)$$

that is, the resistance of the galvanometer should equal the resistance external to the galvanometer, being the joint resistance of the two parallel branches $(a+b)$ and $(c+d)$. Also it may be proved that the best arrangement of the battery is obtained when its resistance equals the external resistance, that is,

$$f = \frac{(a+c)(b+d)}{a+b+c+d} = c \cdot \frac{b+d}{c+d}, \quad \cdot \quad \cdot \quad \cdot \quad (4)$$

the joint resistance of the two parallel branches $(a+c)$ and $(b+d)$.

(In passing, I may notice that Schwendler, in the paper above referred to, and also in a later one in the Philosophical Magazine for January 1867, has assumed it to be necessary for the battery resistance to be very small, in order that the relation exhibited in equation (3) may be satisfied. This appears to me to be totally unnecessary; for the resistance external to the galvanometer when a balance is obtained is quite independent of f, the battery resistance. In fact the proper resistance for the battery when it is to be most advantageously used is given by equation (4).)

As in the present paper we are only concerned with such values of a, b, c, and d as produce a balance, or nearly so, one of these four resistances may be eliminated at once. Let it be a. Then

$$\frac{(a+b)(c+d)}{a+b+c+d} = b \cdot \frac{c+d}{b+d},$$

$$\frac{(a+c)(b+d)}{a+b+c+d} = c \cdot \frac{b+d}{c+d},$$

and

$$a+b+c+d = \frac{(b+d)(c+d)}{d}.$$

Substituting these in equation (2), we get

$$u = E \times \frac{\dfrac{(ad-bc)\,d}{(b+d)\,(c+d)}}{\left\{ b\dfrac{c+d}{b+d} + e \right\} \cdot \left\{ e\dfrac{b+d}{c+d} + f \right\}}$$

$$= E d \times \frac{ad-bc}{(bc+ef)(b+d)(c+d) + ce(b+d)^2 + bf(c+d)^2}. \quad . \quad (5)$$

Now c, d, e, and f being fixed, and b the variable, we have to make u a maximum. As Ed is constant, it may be dismissed. As to the numerator $(ad-bc)$, it vanishes when at a balance; but of course such a thing as an exact balance is unattainable. Let $d \pm \Delta$ be the real value of the resistance we are measuring, d being the calculated value $\dfrac{bc}{a}$, and Δ a small difference, then

$$a(d \pm \Delta) - bc = \pm a\Delta.$$

Therefore the numerator varies as a or as b, since in the present case a and b vary together. Hence we may write b for $(ad-bc)$. Thus

$$u = \frac{b}{(bc+ef)(b+d)(c+d) + ce(b+d)^2 + bf(c+d)^2}.$$

By differentiation and putting $\dfrac{du}{db} = 0$, we obtain

$$(bc+ef)(b+d)(c+d) + ce(b+d)^2 + bf(c+d)^2 = bc(b+d)(c+d)$$
$$+ b(bc+ef)(c+d) + 2bce(b+d) + bf(c+d)^2;$$

therefore

$$ef(b+d)(c+d) + ce(b+d)^2 = b(bc+ef)(c+d) + 2bce(b+d),$$
$$def(c+d) + ce(b+d)^2 = b^2c(c+d) + 2bce(b+d),$$
$$b^2c(c+d+e) = de(cd+df+fc),$$

which gives the relation sought,

$$b = \sqrt{\frac{d}{c} \cdot \frac{cd+df+fc}{c+d+e}} \cdot e; \quad . \quad . \quad . \quad (6)$$

and as $a = \dfrac{bc}{d}$, therefore

$$a = \sqrt{\frac{c}{d} \cdot \frac{cd + df + fc}{c + d + e}} \cdot e. \quad \cdot \quad \cdot \quad \cdot \quad (7)$$

These values of a and b will be found to make $\dfrac{d^2u}{db^2}$ negative; therefore they give the most sensitive arrangement for the fixed values of c, d, e, and f.

If b vary from nothing upwards, it will be found that u rapidly increases up to its maximum value and then slowly decreases, from which it may be concluded that it is better to use too large values of a and b than too small.

In case $c = d$, formulæ (6) and (7) become

$$a = b = \sqrt{ce\frac{c + 2f}{2c + e}}. \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (8)$$

As a numerical example of these formulæ, suppose the resistance to be measured $d = 1000$ ohms, the galvanometer $e = 500$ ohms, the battery resistance $f = 100$ ohms, and we make $c = 1000$ ohms; then the best values for a and b will be found to be $\sqrt{240,000} = 100\sqrt{24}$, or nearly 500 ohms.

Having thus determined the relations of a and b to c, d, e, and f, the latter resistances being fixed, we now proceed to the second part of the problem, to determine the best values of a, b, and c when only d, e, and f are given. This is the case which occurs so often in practice, when we have a battery, a galvanometer, and a resistance to be measured, and three sides of a bridge to which we may give any values we choose (within certain limits).

Insert the values of a and b, as given in equations (6) and (7), in equation (5); then, after some reductions, we obtain $u =$

$$\frac{ad - bc}{2de(cd + df + fc) + \{(c + d + e)(cd + df + fc) + cde\}\sqrt{\dfrac{d}{c} \cdot \dfrac{cd + df + fc}{c + d + e}} \cdot e}$$

We must now consider c the independent variable, a and b being dependent variables. $(ad - bc)$ still varies as a. It does not, however, vary as b, but as the product bc or ad, since d is constant. Therefore we may put the known value of bc in the numerator instead of $(ad - bc)$. Thus $u =$

$$\frac{\sqrt{cde\dfrac{cd + df + fc}{c + d + e}}}{2de(cd + df + fc) + \{(c + d + e)(cd + df + fc) + cde\}\sqrt{\dfrac{d}{c} \cdot \dfrac{cd + df + fc}{c + d + e}} \cdot e}$$

Multiply numerator and denominator by $\sqrt{\dfrac{c}{de} \cdot \dfrac{c+d+e}{cd+df+fc}}$, and we have

$$u = \frac{c}{2\sqrt{cde(c+d+e)(cd+df+fc)} + (c+d+e)(cd+df+fc) + cde},$$

which has to be made a maximum. Differentiating and putting $\dfrac{du}{dc} = 0$,

$$2\sqrt{cde(c+d+e)(cd+df+fc)} + (c+d+e)(cd+df+fc) + cde$$
$$= \frac{c}{\sqrt{cde(cd+df+fc)(c+d+e)}}$$
$$\times \{cde(cd+df+fc) + cde(c+d+e)(d+f) + de(c+d+e)(cd+df+fc)\}$$
$$+ c(c+d+e)(d+f) + c(cd+df+fc) + cde.$$

Therefore

$$2\sqrt{cde(c+d+e)(cd+df+fc)} + df(d+e) - c^2(d+f)$$
$$= \frac{cde\{c(cd+df+fc) + c(c+d+e)(d+f) + (c+d+e)(cd+df+fc)\}}{\sqrt{cde(cd+df+fc)(c+d+e)}}.$$

Multiplying both sides of this equation by the denominator on the right-hand side and reducing, we get

$$\{df(d+e) - c^2(d+f)\}\sqrt{cde(c+d+e)(cd+df+fc)}$$
$$= cde\{c^2(d+f) - df(d+e)\},$$

which is satisfied by

$$df(d+e) - c^2(d+f) = 0,$$

which gives the required relation,

$$c = \sqrt{df\frac{d+e}{d+f}}; \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (9)$$

that is, c equals the square root of the product of the joint resistance of the battery and the resistance to be measured, into the sum of the resistance of the galvanometer and the resistance to be measured. Inserting this value of c in (6) and (7), we find the values of a and b to be

$$a = \sqrt{ef}, \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (10)$$

$$b = \sqrt{de\frac{d+f}{d+e}}. \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (11)$$

In using the Wheatstone's bridge for measuring very high resistances, as, for instance, the insulation resistances of (good)

telegraph lines, the battery resistance is usually very small in comparison with that of the line; hence $\dfrac{df}{d+f}$ will be very little different from f. When this is the case, formula (9) becomes

$$c = \sqrt{f(d+e)}.$$

If also the galvanometer resistance is small compared with the resistance to be measured, then these equations are sufficient for the determination of b and c,

$$b = \sqrt{de},$$
$$c = \sqrt{df}.$$

As a numerical example of these formulæ, suppose $f = 100$ ohms, $e = 1000$ ohms, and d is known to be about 1,000,000 ohms. Then by (10),

$$a = \sqrt{100,000} = 316 \text{ ohms.}$$

By (9),

$$c = \sqrt{\frac{10^6 \times 10^2}{10^6 + 10^2}(10^6 + 10^3)} = 10004 \text{ ohms,}$$

$$b = \frac{ad}{c} = 31608 \text{ ohms.}$$

These values of a, b, and c will be the best. The more convenient arrangement,

$$a = 300, \qquad c = 10,000,$$
$$b = 30,000, \quad d = 1,000,000,$$

would be very nearly the best.

It appears to me that the formulæ (9), (10), and (11), or those following, will be found of considerable practical value. If the same battery and galvanometer be always used, the side a of the bridge will be a constant resistance, and a Table of the nearest convenient values of b and c could be easily calculated for different values of d. Formula (3), which is Schwendler's, can evidently have only a very limited application, as, for instance to the construction of galvanometers for particular purposes. Formula (4) could be sometimes used; but it is a troublesome thing to make combinations of cells for "quantity" or "intensity," besides spoiling them if they are not all precisely similar.

In conclusion, if, to measure a certain resistance, the best resistances for the galvanometer, battery, and the three sides a, b, and c were required, then we should have to make $a = b = c = d = e = f$, which can be proved by combining equations (3), (4), (6), and (10). This, however, is more curious than useful.

XVI. *The Chemistry of Sulphuric Acid-manufacture.*
By H. A. Smith.

[Continued from p. 37.]

Section III. *On the Temperature at which Nitric Acid acts upon Sulphurous Acid.*

IN Section I. I gave one or two laboratory experiments showing some of the conditions under which these gases act upon each other. I now wish to show the temperature at which this action takes place. These experiments were made in a similar manner to the former. I took the glass globe formerly used; and into this the mixed gases were led with the addition of a little water; it was then placed in another vessel containing cold water, arranged so that it could be raised to any required temperature, or boiled if necessary. A thermometer communicated with the interior, its bulb being nearly in contact with the water at the bottom of the globe. The temperature also of the exterior water was accurately observed. At the commencement of the experiment the temperatures were :—

Experiment I.

	Fahrenheit.
Interior of globe . . .	36·7
Exterior water	40·3

the interior being thus a little cooler than the exterior. The water was now cautiously and slowly heated, the temperature being observed from time to time, whilst the first formation of acid in the vessel was carefully noted, the results being :—

Experiment II.

Minutes.	Outside water (Fahrenheit).	Inside globe (Fahrenheit).	Remarks.
Commencement .	40·3	36·7	No action, ruddy fumes.
2	. 62·4	39	„ „
4	. 127·6	122·8	{ Ruddy fumes begin to disappear.
6	. 154·3	200·2	{ Remarkably quick and energetic action.

The globe was now withdrawn from the hot water in which it was and again plunged in cold; the temperature soon fell to 81°·5 Fahr.; but no change took place in the action, that continuing as active as ever. After it had been left in the water some time, it was seen that the great fall in temperature was only temporary; it soon began to rise.

Experiment III.

Minutes.	Outside water (Fahrenheit).	Inside globe (Fahrenheit).
Commencement . .	45·6	81·5
2	45·6	92·3
6	45·9	96·6

at which temperature it remained till the end of the experiment. I find, then, from this experiment that at 200° F. action commences, that at that temperature the sulphurous acid begins to act upon the nitric acid, whilst the second experiment shows that the heat developed by the action itself is pretty considerable.

The globe was now left for twenty-four hours in the cold water (see exp. II.), and after that time had elapsed the contents were submitted to analysis. I here give the result of three analyses :—

	per cent.
Sulphurous acid . .	6·21
Nitric acid	..
Sulphuric acid . . .	93·91
	100·12

the temperatures at time of this analysis being

Fahrenheit.

Inside globe . . .	46·9
Outside water . . .	47·3

In the above experiments it is seen that the temperature never rose to the point of boiling water, but that that degree was very nearly approached.

Section IV. *The Distribution of Heat in the Lead Chamber.*

We have seen in the preceding section that 200° F. is the temperature at which nitric acid begins to act upon sulphurous acid. I now wish to show the temperature of the lead chamber in which the preceding action takes place on a large scale, and then to see if I can draw some conclusion as to the best temperature at which to keep the sulphuric acid-chamber.

In order to obtain a good idea of the temperature, I took daily observations at different points in the chamber during a year, and have condensed the results obtained into the form of diagrams.

Ordinary maximum and minimum thermometers were employed; but instead of being fixed, as usual, upon a wooden back, a *glass* back was employed, upon which the degrees were etched, and the thermometers fixed thereto by thick platinum wire, thus having instruments capable of resisting all acids.

These thermometers were lowered into the chamber at the different points by long "threads" of lead and allowed to remain for about two hours, the yield of vitriol and general appearance of the chamber being carefully noted each day. The results obtained by this investigation have been very completely borne out by those already shown in Section III., which, although I considered their proper place in the paper to be before the present section, nevertheless came later in the course of investigation.

As in a former case, I divided the chamber into separate parts, so that I could have some definite plan of procedure. In this case the chamber was divided along its length at four heights, thus—

1st, at the height of 24 feet from bottom of chamber,
2nd,   ,,   ,,   15   ,,   ,,   ,,   ,,
3rd,   ,,   ,,   8   ,,   ,,   ,,   ,,
4th,   ,,   ,,   3   ,,   ,,   ,,   ,,

the temperature being taken every 10 feet along the length at these heights.

In looking at Diagram I. the first thing that strikes us is the very sudden fall in temperature which takes place in passing from 10 feet from the end of chamber (at the entrance) to 20 feet, there being a fall here of 87° F. After this the temperature is comparatively constant till it reaches 110 feet from end, when it again falls continually till it reaches 113° F., at which temperature the gas passes from the chamber. This is that portion of the chamber which I have previously called the "reservoir," and in which very little, if any, action between the gases takes place; and it is worthy of notice that, with the exception of the first ten feet, at no place in this portion does the temperature rise above 130° F. And if we turn to the previous section of this paper, we find that in exp. II. the nearest approach to this temperature is 122°·8 F.; and here we have the remark, "ruddy fumes begin to disappear." Here, then, is another proof, if another were required, that the upper part of the lead chamber is not of use as a condensing "space," but merely as a reservoir for containing the gases, and, *if necessary,* assisting proper mixture.

In this diagram we may see also the points in the chamber at which steam is thrown in. At 40 feet and at 70 and 110 feet respectively we have decided falls of temperature, these being very nearly the places of the steam-pipes. The high temperature at the beginning may be accounted for by the fact that shortly below this the pipe by which the gases are conducted to the chamber is inserted.

Length of chamber in feet.

10 20 30 40 50 60 70 80 90 100 110 120 130 140
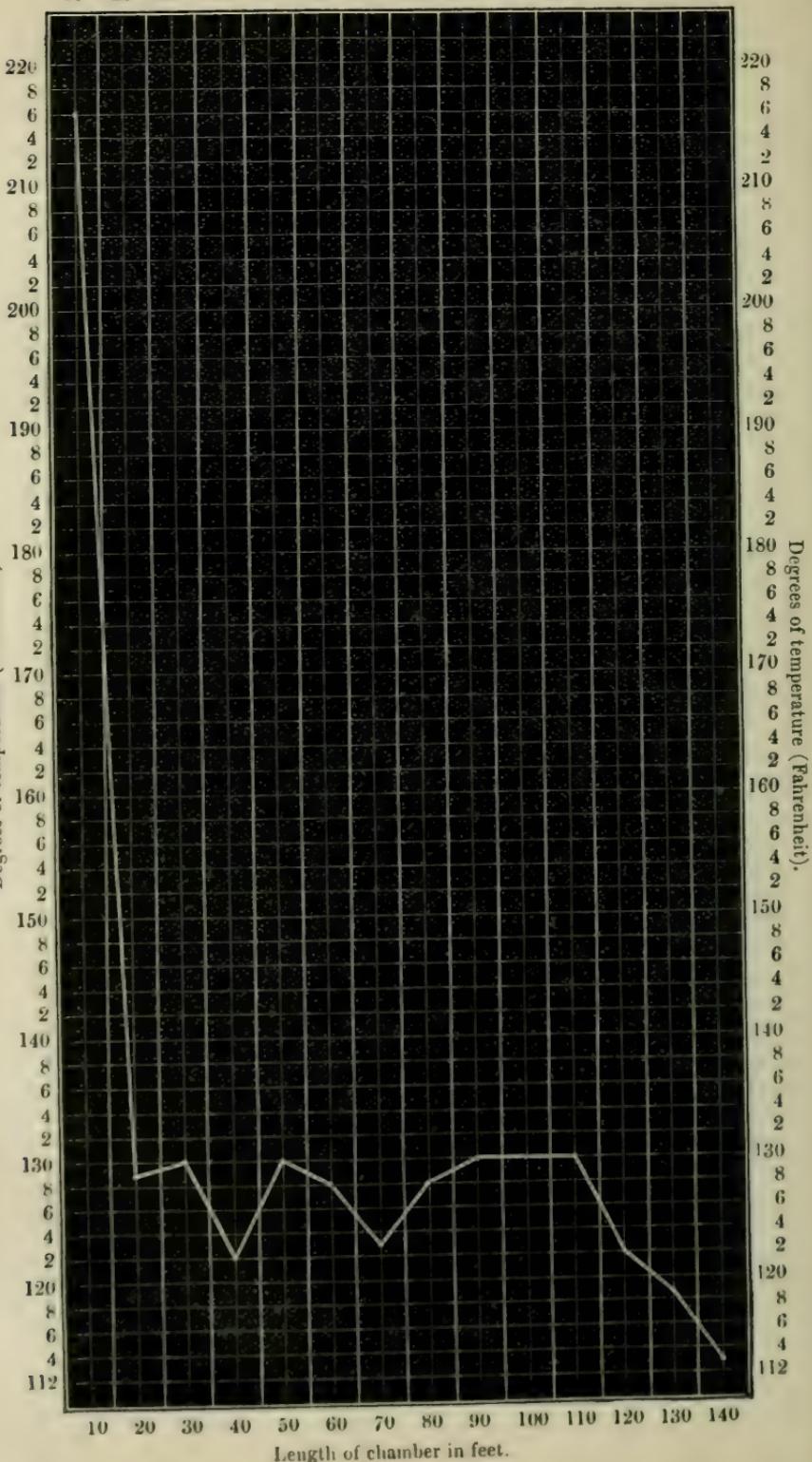
Diagram I.—Heat of Chamber at 2.4 feet from bottom.

Degrees of temperature (Fahrenheit).

Degrees of temperature (Fahrenheit).

220
8
6
4
2
210
8
6
4
2
200
8
6
4
2
190
8
6
4
2
180
8
6
4
2
170
8
6
4
2
160
8
6
4
2
150
8
6
4
2
140
8
6
4
2
130
8
6
4
2
120
8
6
4
112

10 20 30 40 50 60 70 80 90 100 110 120 130 140

Length of chamber in feet.

Diagram II.—Heat of Chamber at 15 feet from bottom.

Degrees of temperature (Fahrenheit).

Degrees of temperature (Fahrenheit).
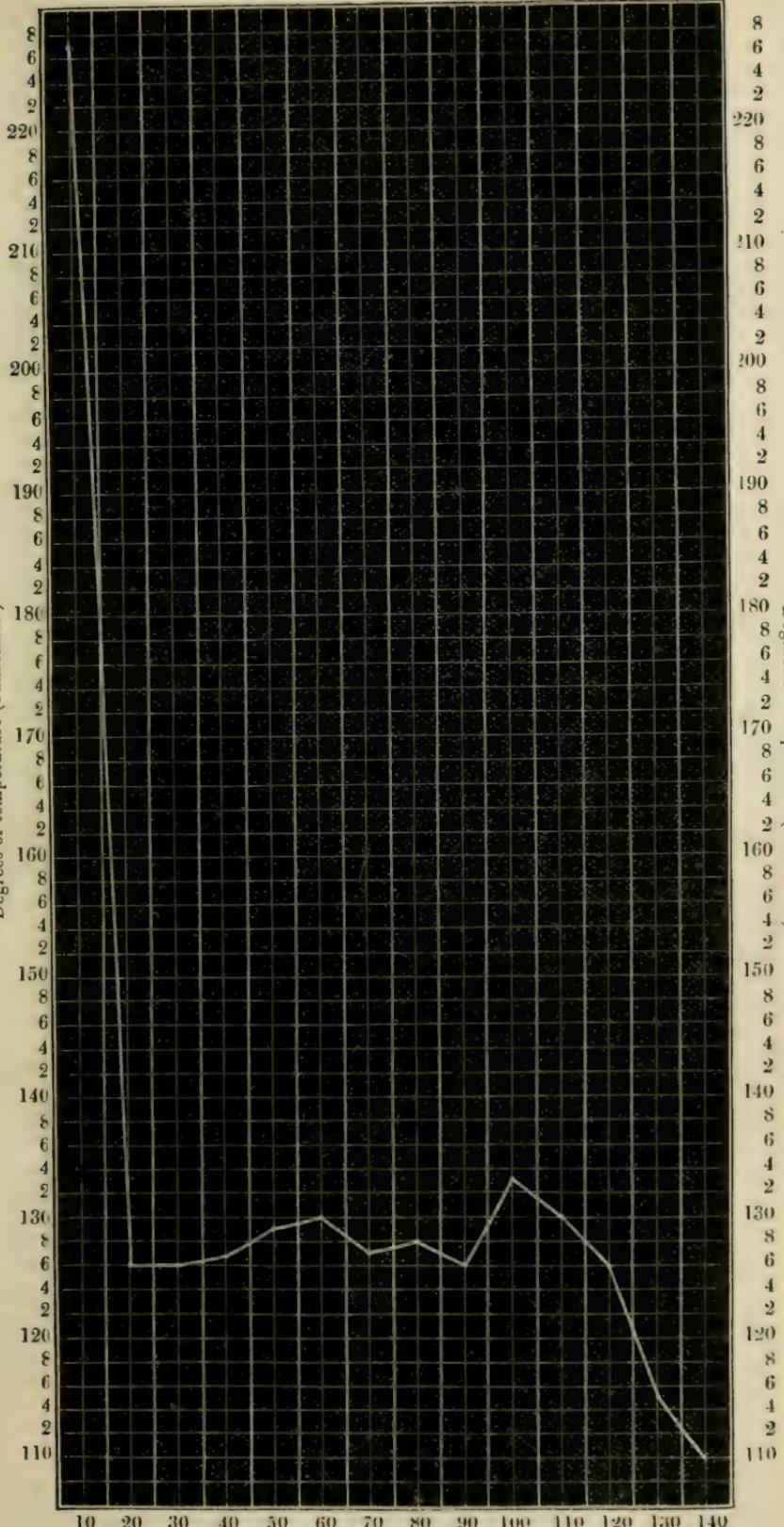
Little is to be said respecting this diagram which has not been said on the former. In this also is noticeable the great fall in temperature which has taken place from 10 to 20 feet from end of chamber; in this case the fall is even greater than before, changing here from 227° F. to 126° F., a difference of 101° F. This is accounted for by the fact that the entrance-pipe is much nearer this point than the former, being in fact just above the spot where the temperature was observed. The regularity of the heat after this sudden fall is also, as in the former case, very remarkable—the variation being between 126° and 133° F. till we reach 120 feet from end of chamber, when the temperature falls to 110° F., the average temperature, however, being about 126° to 128° F.,—in this case also bearing out the fact (by reference to exp. II., previous section) that the upper part of the chamber is unnecessary.

Diagram III.

Heat of Chamber 8 feet from bottom.

Length of chamber in feet.

Diagram IV.
Heat of chamber 3 feet from bottom.

Length of chamber in feet.

Length of chamber in feet.
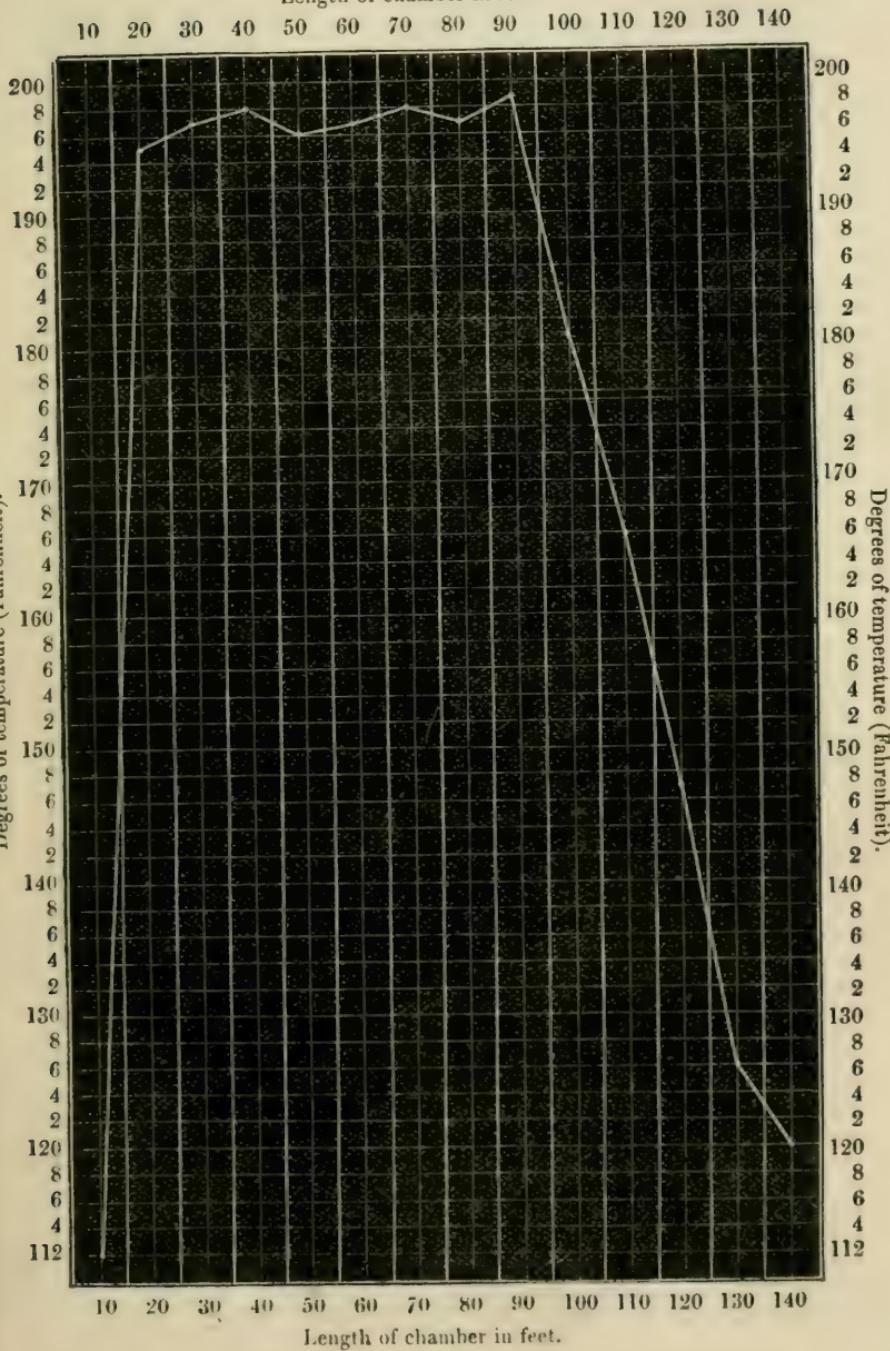
In Diagram III. we find a higher average of temperature, varying for the most part between 150° and 166° F.; whilst at entrance and exit the temperature is comparatively low, rising towards the centre of the chamber. Here, now, we approach very nearly the required temperature, 200° F. being the observed degree at which nitric acid acted on sulphurous acid; and by reference to my former paper we find that this portion of the chamber is really the "working" portion. It is necessary also to observe along with this diagram the next, Diagram IV. In my former paper I showed experimentally, by analyses of the gases at 3 feet from bottom of chamber, that the greatest amount of action went on at that portion; and I have shown also (Section III.) that the temperature at which nitric acid began to act on sulphurous acid was 200° F.; so that now by observing these two diagrams we see how closely the laboratory and the manufacturing results agree. I have already spoken of this in Diagram III.; but it is much more distinctly observed in Diagram IV. Beginning at 112° F., the temperature rises suddenly till at 20 feet from entrance it attains 195° F., *whilst the temperature is for the most part from* 195° *to* 199° F. After 90 feet from entrance of chamber it falls at an almost regular amount of 20 degrees for each 10 feet of chamber length, until at 140 feet from entrance of chamber the temperature is 120° F.

Here, then, we have an example of almost perfectly suitable temperature. At this time also the yield of vitriol obtained from this chamber was as nearly the theoretical amount as could practically be obtained; and it was found that whenever the temperature of the chamber was allowed to increase or diminish the result was bad. At this time the amount of nitric acid escaping was almost *nil*, whilst the colour of the liquid coming from the Gay-Lussac tower showed that the gas escaping was really NO^2 with a scarcely appreciable amount of nitric acid. (I may say here that whenever the colour of this liquor is of a *dark* red colour, it is a sign of the escape either of nitric acid or some of the higher oxides of nitrogen.)

The conclusions I draw from these and the preceding investigations may be summed up thus :—

1. The best form of chamber to be employed is one which is long and not high, the analyses pointing to one of somewhat the following dimensions—150 feet long, 25 or 30 feet wide, and about 10 or 12 feet high. We have thus a large condensing surface, the mixed gases coming readily into contact with all parts of the chamber, whilst they are also in contact with the previously condensed acid which rests on the sides of the chamber.

2. The temperature of the chamber should be kept as nearly

as possible about 200° F.,—this also acting as a regulator for the amount of steam thrown into the chamber.

3. That in " starting " a chamber, sulphuric acid should be run on the bottom in preference to water, which is at present generally employed.

XVII. *On Fractional Distillation.* By J. ALFRED WANKLYN, *Corresponding Member of the Royal Bavarian Academy of Sciences*[*].

IT is an extraordinary fact that there is, up to the present time, no theory of fractional distillation. Chemists are perfectly aware that, on distilling a mixture of two unequally volatile liquids, they will find some of the less volatile liquid in an early portion of the distillate, and some of the more volatile liquid in a later portion of the distillate. And, on the other hand, chemists are quite as well aware that, notwithstanding this clinging together of different liquids, almost perfect separations are attainable when fractional distillation is laboriously and systematically practised. Every chemist's own experience supplies instances of the kind ; and most of us have our own peculiar methods of performing the operation. My object, on the present occasion, is to lay down a general theory of this process, and to explain how it comes to pass that it is at once so laborious and yet so successful when duly performed.

In order to gain a clear idea of the effect of admixture, let the hypothetical case of two liquids of precisely the same volatility be supposed. In such a case each fraction of the distillate will have the same composition as every other fraction and as the original mixture. If the two liquids constituting the mixture be named A and B respectively, and if the relative quantities of them existing in the original mixture be expressed by a and b, then $a : b$ will express the ratio in which A and B are present in the original liquid and in every fraction of the distillate from first to last. If we picture to ourselves the course of events on heating such a mixture, we perceive that, on applying heat to it, the quantity of heat which is received by each constituent is exactly proportional to the quantity of each constituent present in the mixture. If a and b be the relative quantities of A and B, then a and b will also be the relative quantities of heat received by A and B in a given period of time. In the case under consideration the volatilities of A and B are supposed to be equal ; therefore a and b, which express the original quantities of A and B, and which (as just shown) express the relative quantities of

heat received by A and B, must also express the relative quantities of A and B in the vapour evolved on boiling, and consequently in the distillate.

Now let us pass from the hypothetical case of liquids of equal volatility to the actual cases of liquids of unequal volatilities. In such cases the composition of the vapour which escapes when the mixture is boiled will be different from the composition of the original mixture; and putting v for the coefficient of volatility of A, and v' for the coefficient of volatility of B, we have av and bv' for the relative quantities of A and B which escape in the form of vapour on distilling the mixture. If we put δ for an infinitely small first distillate, we have $\dfrac{av\delta}{av + bv'}$ for the quantity of A in the first unit of distillate, and $\dfrac{bv'\delta}{av + bv}$ for the quantity of B in the first unit of distillate. Collecting the formulæ, we have:—

$a + b$ = relative quantities of A and B in the original mixture.

$av + bv'$ = relative quantities of A and B in the first unit of distillate.

$\dfrac{av\delta}{av + bv'}$ = quantity of A in first unit of distillate.

$\dfrac{bv'\delta}{av + bv'}$ = quantity of B in first unit of distillate.

After the escape of the first unit of distillate ($=\delta$) the composition of the liquid left behind in the retort will have altered, and the relative quantities of A and B will now be represented not by a and b, but by the formulæ

$$\left(a - \frac{av\delta}{av + bv'} \right)$$

and

$$\left(b - \frac{bv'\delta}{av + bv'} \right).$$

In order to find the values of A and B in the second unit of distillate, these formulæ must be substituted for a and b. It will be obvious that as the distillation progresses, the composition of the successive units of distillate must progressively alter. If v be greater than v', then the quantity of A in the units of distillate must progressively diminish; if v be less than v', the quantity of A in the units of distillate must progressively increase. Obviously, too, the greater the difference between v and v', the more rapid the alteration in the composition of successive units of distillate.

Such, I believe, is the real march of the process of fractional distillation; and these formulæ offer an explanation why by frequent repetitions and judicious management the chemist is able to effect almost perfect separations by means of fractional distillation. The coefficient of volatility, however, is something more than the tension of the vapour at the boiling-point of the mixture. Ten years ago I showed that the density of the vapour had to be taken into account (and Berthelot has also independently, but, I believe, subsequently, insisted on the necessity of regarding vapour-density in judging of relative volatility). The coefficient of volatility comprises at least the tension of the vapour and the density of the vapour.

The heat rendered latent during evaporation must also be represented in this coefficient. Possibly, too, adhesion between the liquids will require representation in certain classes of instances; and this will cause difficulty and complication. I am of opinion, however, that in the vast majority of cases the conjoined influences of tension, density, and latent heat will adequately express the coefficient of volatility. If t be put for the tension and d for the vapour-density, then I propose $v = td^2$ for the value of the coefficient of volatility. This will be more fully explained on a future occasion. At present my main object is to insist on the fact that, when distilling in mixture, different liquids have different coefficients of volatility, and that the composition of successive portions of distillate is governed by these coefficients and by the original proportions of the liquids present in the mixture, in the manner set forth.

I have made some experiments on mixtures of A and B wherein the quantity of A is overwhelmingly greater than that of B. This disposition of the quantities offers certain advantages for investigating the general march of fractional distillation.

When a is immense compared with b, $\dfrac{b}{a+b}$ is undistinguishable from $\dfrac{b}{a}$. And putting $v = 1$, when a is immense compared with b we get $\dfrac{bv'}{av + bv'}$ undistinguishable from $\dfrac{bv'}{a}$. I have made four experiments, wherein

	a	$+$	b
Exp. I. . .	1000000	$+$	1000
Exp. II. . .	1000000	$+$	1·00
Exp. III. . .	1000000	$+$	0·50
Exp. IV. . .	1000000	$+$	0·20

In these cases the quantity of b in a unit of the original mix-

ture is $\dfrac{b}{a+b}$ undistinguishable from $\dfrac{b}{a}$; viz.

$$\text{Exp. I.} \quad \cdot \quad \cdot \quad \frac{1000}{1001000} \text{ or } \frac{1000}{1000000}$$

$$\text{Exp. II.} \quad \cdot \quad \cdot \quad \frac{1}{1000001} \text{ or } \frac{1}{1000000}$$

$$\text{Exp. III.} \quad \cdot \quad \frac{0 \cdot 50}{1000000 \cdot 5} \text{ or } \frac{0 \cdot 50}{1000000}$$

$$\text{Exp. IV.} \quad \cdot \quad \frac{0 \cdot 20}{1000000 \cdot 2} \text{ or } \frac{0 \cdot 20}{1000000}$$

In like manner in these instances the quantity of B in a unit of distillate is expressed indifferently either by $\dfrac{bv'}{av+bv'}$ or by $\dfrac{bv'}{a}$. Now, if it were possible to distil off and work with an infinitesimally small quantity of distillate, we should get $\dfrac{bv'}{a}$ for the value of B in a unit of distillate; whence the relation of the value of B in a unit of original liquid to the value of B in a unit of distillate is very simple, viz. $\dfrac{b}{a} : \dfrac{b}{a} v'$. Although we cannot distil off and work with an infinitely small fraction of distillate, yet we can, in a series of experiments, distil off and work with equal fractions (which will be equal numbers of the infinitesimal fractions). In each of the four experiments I took a kilogramme of A and B, and I distilled off 50 grammes (or one twentieth of the whole). The results were as follows :—

$$A = \text{water.} \quad B = \text{ammonia.}$$

		Quantity of ammonia contained in a litre of original liquid.	Quantity of ammonia contained in the first 50 cubic. centims of distillate.
		milligrms.	milligrms.
Exp.	I. . .	1000	480
Exp.	II. . .	1·00	0·50
Exp.	III. . .	0·50	0·235
Exp.	IV. . .	0·20	0·095

Now on inspection it will be manifest that, within the limit of experimental error, the strength of the distillate is proportional to the strength of the original liquid. In each of these four experiments 1 litre of liquid was taken and 50 cubic centims. distilled off; that is to say, one twentieth of the whole was distilled. If instead of 50 cubic centims. I distil off 100 cubic centims., I also find that the strength of the distillate is proportional to the

strength of the original liquid; and it is a fact that in the case of dilute solutions of ammonia, if the same fraction be distilled off, the strength of that fraction is proportional to the strength of the original liquid. I cannot but regard this as a result of extreme importance, and, bearing in mind the immense range over which it holds good, consider that it affords proof that the general march of fractional distillation is as has been represented.

Putting $v=1$, let us endeavour to make out the value of v' from the four experiments. Inasmuch as the quantity of distillate was 50 cubic centims., and the quantity of original liquid was 1 litre, we must multiply the ammonia in the distillate by 20 in order that it may be comparable with that in the original liquid. Having done this, we see that the distillate is 9·6 times as strong as the original liquid. If the 50 cubic centims. were an infinitesimal fraction of distillate, then v' would equal 9·6; but the 50 cubic centims. is so large a fraction, that during the evolution of it the liquid has gone down in strength from 1000 milligrammes of ammonia per litre to about 547 milligrammes per litre. During the distillation of this 50 cubic centims. the strength of the successive portions of distillate must have varied so that the ratio of the strength of the first infinitesimal to the strength of the last infinitesimal shall be 1000 : 520. The real value of v' is therefore between 13 and 14.

It is my intention to make determinations of the " coefficients of volatility " of different liquids.

XVIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 76.]

June 13, 1872.—Sir John Lubbock, Bart., Vice-President, in the Chair.

THE following communication was read :—

" On the Spectrum of the Great Nebula in Orion, and on the Motions of some Stars towards or from the Earth." By William Huggins, LL.D., D.C.L., F.R.S.

In my early observations of the spectrum presented by the gaseous nebulæ, the spectroscope with which I determined the coincidence of two of the bright lines respectively with a line of nitrogen and a line of hydrogen was of insufficient dispersive power to show whether the brightest nebular line was double, as is the case with the corresponding line of nitrogen.

Subsequently I took some pains to determine this important point by using a spectroscope of greater dispersive power. I found, however, that the light furnished by the telescope of eight inches aperture, to which the spectroscope was attached, was too feeble, even in

the case of the brightest nebulæ, to give the line with sufficient distinctness when a narrow slit was used. The results of this later examination are given in a paper I had the honour of presenting to the Royal Society in 1868. I there say *:—" I expected that I might discover a duplicity in the line in the nebula corresponding to the two component lines of the line of nitrogen ; but I was not able, after long and careful scrutiny, to see the line double. The line in the nebula was narrower than the double line of nitrogen ; this latter line may have appeared broader in consequence of irradiation, as it was much brighter than the line in the nebula." When the spark was placed before the object-glass of the telescope, the light was so much weakened that one line only was visible in the spectroscope. " This line was the one which agrees in position with the line in the nebula, so that under these circumstances the spectrum of nitrogen appeared precisely similar to the spectra of those nebulæ of which the light is apparently monochromatic. This resemblance was made more complete by the faintness of the line ; from which cause it appeared narrower, and the separate existence of its two components could no longer be detected. When the line was observed simultaneously with that in the nebula, it was found to appear but a very little broader than that line." I also remark :—" The double line in the nitrogen-spectrum does not consist of sharply defined lines, but each component is nebulous, and remains of a greater width than the image of the slit. The breadth of these lines appears to be connected with the conditions of tension and temperature of the gas. Plücker† states that when an induction-spark of great heating-power is employed, the lines expand so as to unite and form an undivided band. Even when the duplicity exists, the eye ceases to have the power to distinguish the component lines, if the intensity of the light be greatly diminished." I state further :—" I incline to the belief that it [the line in the nebula] is not double."

One of the first investigations which I proposed to myself when, by the kindness of the Royal Society, I had at my command a much more powerful telescope, was the determination of the true character of the bright line in the spectra of the nebulæ which is apparently coincident with that of nitrogen. From various circumstances, chiefly connected with the alterations and adjustments of new instruments, I was not able to accomplish this task satisfactorily until within the last few months.

Description of Apparatus.

It seems to me desirable to give a description of the spectroscopic apparatus with which the observations in this paper were made. In the former paper, to which I have already referred, I gave some reasons‡ to show that the ordinary method of comparison, by reflecting light into the spectroscope by means of a small prism placed before one half of the slit, is not satisfactory for very delicate observations unless certain precautions are taken. I then describe an

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* Phil. Trans. 1868, pp. 542, 543.   † Ibid. 1865, p. 13.
‡ Ibid. 1868, pp. 537, 538.

arrangement for this purpose, which, with one or two modifications, is adopted in the collimator constructed for use with the Royal Society's telescope. I give the description from that paper* :—

"The following arrangement for admitting the light from the spark appeared to me to be free from the objections which have been referred to, and to be in all respects adapted to meet the requirements of the case. In place of the small prism, two pieces of silvered glass were securely fixed before the slit at an angle of 45°. In a direction at right angles to that of the slit, an opening of about $\frac{1}{10}$ inch was left between the pieces of glass for the passage of the pencils from the object-glass. By means of this arrangement the spectrum of a star is seen accompanied by two spectra of comparison, one appearing above and the other below it. As the reflecting surfaces are about 0·5 inch from the slit, and the rays from the spark are divergent, the light reflected from the pieces of glass will have encroached upon the pencils from the object-glass by the time they reach the slit, and the upper and lower spectra of comparison will appear to overlap to a small extent the spectrum formed by the light from the object-glass. This condition of things is of great assistance to the eye in forming a judgment as to the absolute coincidence or otherwise of lines. For the purpose of avoiding some inconveniences which would arise from glass of the ordinary thickness, pieces of the thin glass used for the covers of microscopic objects were carefully selected ; and these were silvered by floating them upon the surface of a silvering solution. In order to ensure that the induction-spark should always preserve the same position relatively to the mirror, a piece of sheet gutta percha was fixed above the silvered glass ; in the plate of gutta percha, at the proper place, a small hole was made of about $\frac{1}{20}$ inch diameter. The ebonite clamp containing the electrodes is so fixed as to permit the point of separation of these to be adjusted exactly over the small hole in the gutta percha. The adjustment of the parts of the apparatus was made by closing the end of the adapting-tube, by which the apparatus is attached to the telescope, with a diaphragm with a small central hole, before which a spirit-lamp was placed. When the lines from the induction-spark, in the two spectra of comparison, were seen to overlap exactly, for a short distance, the lines of sodium from the light of the lamp, the adjustment was considered perfect. The accuracy of adjustment has been confirmed by the exact coincidence of the three lines of magnesium with the component lines of *b* in the spectrum of the moon."

The modifications of this plan consist in the substitution of a thin silver plate polished on both surfaces for the pieces of silvered glass. The opposite side of the silver plate to that from which the terrestrial light is reflected to the slit reflects the images formed by the object-glass to the side of the tube where a suitable eyepiece is fixed. This arrangement forms a very convenient finder ; for it is easy to cause the image of the star to disappear in the hole in the silver plate. When this is the case, the line of light formed by the star falls on the slit, and its spectrum is visible in the spectroscope. This colli-

* Phil. Trans. 1868, p. 538.

mator is so constructed that, by means of a coupling-screw, any one of
three spectroscopes can be conveniently attached to it.

This apparatus performs admirably ; but it seemed to me desirable,
for observations of great delicacy, to be able to dispense with reflec-
tion, and to place the source of the light for comparison directly before
the slit.   Formerly I accomplished this object by placing the spark
or vacuum-tube before the object-glass of the telescope.   The great
length of the present telescope renders this method inconvenient ;
but a more important objection arises from the great diminution
of the light when the spark is removed to a distance of 15 feet from
the slit.   I therefore resolved to place the spark or vacuum-tube
within the telescope at a moderate distance from the slit.   For
this purpose holes were drilled in the tube opposite to each other,
at a distance of 2 feet 6 inches within the principal focus.   Before
these holes short tubes were fixed with screws ; in these tubes slide
suitable holders for carrying electrodes or vacuum-tubes.   The spark
is thus brought at once nearly into the axis of the telescope.   The
final adjustment is made in the following manner :—A bright star is
brought into the centre of the field of an ordinary eyepiece ; the
eyepiece is then pushed within the focus, when the wires or vacuum-
tube can be seen across the circle of light formed by the star out of
focus.   The place of discharge between the electrodes, or the middle
of the capillary part of the vacuum tube, is then brought into the
centre of the circle of light.   The vacuum-tubes are covered with
black paper, with the exception of a space about a $\frac{1}{4}$ inch long in the
middle of the capillary part ; through this small uncovered space the
light passes to reach the slit.

The accuracy of both methods of comparison, that by reflection
and that by the spark within the tube, was tested by the compa-
rison of the three bright lines of magnesium and the double line of
sodium with the Fraunhofer lines b and D in the spectrum of the
moon.   I greatly prefer the latter method, because it is free from
several delicate adjustments which are necessary when the light is
reflected and which are liable to be accidentally displaced.

Spectroscope A is furnished with a single prism of dense glass
with a refracting angle of 59° 42', giving 5° 6' from A to H.

Spectroscope B has two compound prisms of Mr. Grubb's con-
struction, which move automatically to positions of minimum devi-
ation for the different parts of the spectrum.   Each prism gives
about 9° 6' for minimum deviation from A to H.

Spectroscope C is furnished with four similar prisms.

The small telescopes of the three spectroscopes are of the same
size : diameter of object-glass $1\frac{1}{4}$ inch ; each is furnished with three
eyepieces magnifying 5·5, 9·2, and 16·0 diameters.

Spectrum of the Nebula of Orion.

With spectroscopes A and B four[*] lines are seen ; they are repre-
sented in the diagram which accompanies this note.   The scale in
the diagram gives wave-lengths.

    * The fourth line was first seen in nebula 18 H. IV. (Phil. Trans. 1864, p. 441).

First line.—With spectroscope B and eyepiece 1 and 2, the slit being made very narrow, this line was seen to be very narrow, of a width corresponding to the slit, and defined at both edges, and undoubtedly not double. The line of nitrogen when compared with it appeared double, and each component nebulous and broader than the line of the nebula. This latter line was seen on several nights to be apparently coincident with the middle of the less refrangible line of the double line of nitrogen. This observation was on one night confirmed by observation with the more powerful spectroscope C.

The question suggests itself whether, under any conditions of pressure and temperature, the double line of the nitrogen-spectrum becomes single; and further, if this should be found to be the case, whether the line becomes single by the fading out of its more refrangible component, or in what other way the single line of the nebula comes to occupy in the spectrum not the position of the middle of the double line of nitrogen, but that of the less refrangible of the lines.

I stated in my former paper that when for any reason the light from the luminous nitrogen is greatly reduced in intensity, the double line under consideration is the last to disappear, and consequently a state of things may be found in which the light of nitrogen is sensibly monochromatic when examined with a narrow slit*. Under these circumstances the line of nitrogen appears narrower, and the separate components can be detected with difficulty, if at all.

I stated also that the breadth of the component lines appears to be connected with the conditions of density and temperature of the gas. As was to be expected from theoretical considerations, the lines become narrower and less nebulous as the pressure is diminished. My observations of this change seemed to show that the diminution of the breadth of the lines takes place chiefly at the outer sides of the lines; so that in the light from very rarefied gas the double line is narrower, but the space of separation between the components is not as much wider as would be the case if the lines had decreased equally in width on the sides towards each other.

When the pressure of the gas is reduced to about 15 inches of mercury, the line-spectrum fades out to give place to Plücker's spectrum of the first order. During this process a state of things occurs when, for reasons already stated, the spectrum becomes *sensibly* monochromatic when viewed with a narrow slit and a spectroscope of several prisms. The line is narrower but remains double, and has the characters described in the preceding paragraph.

As the pressure is diminished, the double line fades out entirely, and the spectrum of the second order gives place to the spectrum of the first order. When, however, the pressure becomes exceedingly small, from 0·1 inch to 0·05 inch, or less, of mercury, there is a condition of the discharge in which the line again appears, while

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* Phil. Trans. 1868, pp. 540–546. Observations on this point were subsequently made by Frankland and Lockyer (Proc. Roy. Soc. vol. xvii. p. 453). It should be stated that the authors make no reference to this observation, though they refer to a purely hypothetical suggestion contained in the same paper.

the other lines remain very faint. Under these conditions I have always been able, though with some difficulty, on account of the faint light when the necessary dispersive power (spectroscope B with second or third eyepiece) and a narrow slit are used, to see the line to be double ; but it is narrower than when the gas is more dense, and may be easily mistaken for a single line. I have not yet been able to find a condition of luminous nitrogen in which the line has the same characters as those presented by the line in the nebula, where it is single and of the width of the slit.

Upon the whole I am still inclined to regard the line in the nebula as probably due to nitrogen.

If this should be found to be the case, and that the nebular line has originally the refrangibility of the middle of the double line of nitrogen, then we should have evidence that the nebula is moving from the earth. The amount of displacement of the nebular line from the middle of the double line of nitrogen corresponds to a velocity of 55 miles per second from the earth. At the time of observation the part of the earth's orbital motion, which was from the nebula, was 14·9 miles per second. From the remaining 40 miles per second would have to be deducted the probable motion from the nebula due to the motion of the solar system in space. This estimation of the possible motion of the nebula can be regarded as only approximate.

If the want of accordance of the line in the nebula with the middle of the double line of nitrogen be due to a recession of the nebula in the line of sight, there should be a corresponding displacement of the third line as compared with that of hydrogen. For reasons which will be found in a subsequent paragraph, I have not been able to make this comparison with the necessary accuracy.

In my former paper* I gave reasons against supposing so large a motion in the nebula ; these were based on the circumstance that the nebular line falls upon the double nitrogen line, which the present observations confirm. I was not then able to use a slit sufficiently narrow to show that the nebular line is single and not coincident with the middle of the double line of nitrogen.

I am still pursuing the investigation of the parts of this inquiry which remain unsettled.

Second line.—This line was found by my former comparisons to be a little less refrangible than a strong line in the spectrum of barium. Three sets of measures give for this line a wave-length of 4957 on Ångström's scale ; this would show that the line agrees nearly in position with a strong line of iron. At present I am not able to suggest to what substance this line belongs.

This line is also narrow and defined. I suspect that the brightness of this line relatively to the first line varies in different nebulæ.

Third and fourth line.—My former observations show that these lines agree in position with two lines of the spectrum of hydrogen, that at F and the line near G.

These lines are very narrow and are defined ;
the hydrogen therefore must be at a low ten-
sion.

The brightness of these lines relatively to the
first and second lines varies considerably in dif-
ferent nebulæ; and I suspect they may also vary
in the same nebulæ at different times, and even
in different parts of same nebula ; but at present
I have not sufficient evidence on these points*.
I regret that, in consequence of a continuance
of bad weather, I have not yet been able to
obtain decisive observations as to the possible
motion of the nebula in the line of sight.   With
spectroscope B and eyepiece 2, the lines appear to
be coincident with those of hydrogen.   In con-
sequence of the uncertainty of the character of
the first line, which is single, while that of ni-
trogen is double, this determination can now
only be made by means of the comparison of
the third line with that of hydrogen.   This
third line becomes very faint from the great
loss of light unavoidable in a spectroscope that
gives a sufficient dispersive power, and the com-
parison can only be attempted when the sky is
very clear and the nebula near the meridian.

* Since writing this sentence, I have seen a note by
Prof. D'Arrest in the ' Astronomische Nachrichten,'
No. 1885. Speaking of the nebula H. IV. 37, he says :
—" Sein Spectrum ist ausser von Huggins bisher nur
noch von Dr. H. Vogel untersucht worden. In No. 1864,
Ast. Nachr. theilt letzterer mit, dass er im Jahre 1871,
im Widerspruch mit Huggins' Angabe, die Linie Neb.
$(3)=(2)$, bisweilen sogar $(2)<(3)$ gefunden habe.
Nach Huggins war dagegen im Jahre 1864 positiv
$(2)>(3)$.   Ist Vogel's Beobachtung, wie ich nicht be-
zweifle, zuverlässig, so wird seine Vermuthung einer
Veränderung hier in der That begründet sein, denn
diesen Winter, namentlich im Februar und März
1872, fand ich wiederum, so wie es Huggins früher ge-
sehen hat, unzweifelhaft $(2)>(3)$. Die relative Inten-
sität der drei Lichtarten habe ich mehrfach in Zahlen
geschätzt und erhielt z. B. in den letzten Nächten :

	März 6.	März 13.
(1)	100	100
(2)	58	63
(3)	49	52 "
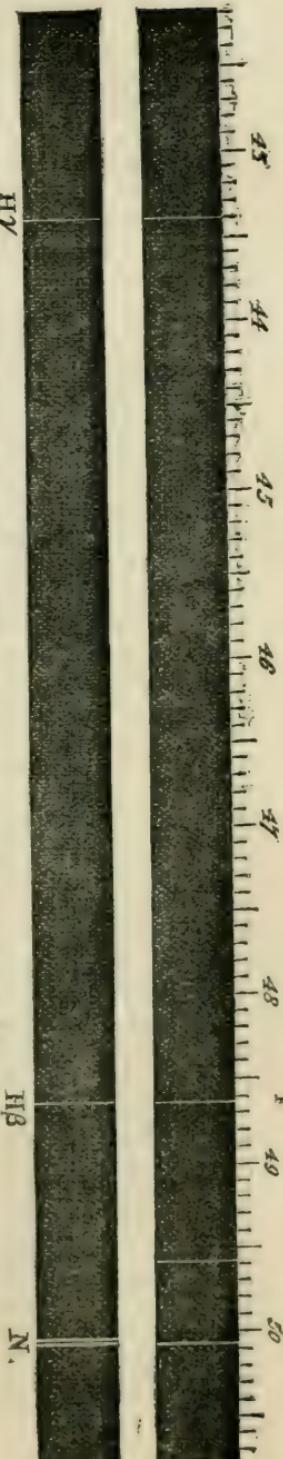
§ 2. *On the Motions of some Stars towards or from the Earth.*

In the early part of 1868 I had the honour of presenting to the
Royal Society some observations on a small change of refrangibility
which I had observed in a line in the spectrum of Sirius as com-
pared with a line of hydrogen, from which it appeared that the star
was moving from the earth with a velocity of about twenty-five miles
per second, if the probable advance of the sun in space be taken
into account [*].

It is only within the last few months that I have found myself in
possession of the necessary instrumental means to resume this
inquiry, and since this time the prevalence of bad weather has left
but few nights sufficiently fine for these delicate observations.

Some time was occupied in obtaining a perfectly trustworthy method
of comparison of the spectra of stars with those of terrestrial sub-
stances, and it was not until I had arranged the spark within the tube,
as described at the beginning of this note, that I felt confidence
in the results of my observations.

It may be well to state some circumstances connected with these
comparisons which necessarily make the numerical estimations given
further on less accurate than I could wish.   Even when spectroscope
C, containing four compound prisms, and a magnifying-power of 16
diameters are used, the amount of the change of refrangibility to be
observed appears very small.   The probable error of these estimations
is therefore large, as a shift corresponding to five miles per second
(about $\frac{1}{10}$ of the distance of D^1 to D^2), or even a somewhat greater
velocity, could not be certainly observed.   The difficulty arising from
the apparent smallness of the change of refrangibility is greatly in-
creased by some other circumstances.   The star's light is faint when
a narrow slit is used ; and the lines, except on very fine nights, cannot
be steadily seen, in consequence of the movements in our atmosphere.
Further, when the slit is narrow, the clock's motion is not uniform
enough to keep the spectrum steadily in view ; for these reasons I
found it necessary to adopt the method of estimation by comparing
the shift with a wire of known thickness, or with the interval be-
tween a pair of close lines.   I found that, under the circumstances,
the use of a micrometer would have given the appearance only of
greater accuracy.   I wish it, therefore, to be understood that I re-
gard the following estimations as provisional only, as I hope, by

[*] Phil. Trans. 1868, pp. 529–550. As a curious instance in which later
methods of observation have been partially anticipated, a reference may be made
to an ingenious paper in the Philosophical Transactions for 1783, vol. lxxiv., by
the Rev. John Michell, entitled " On the means of discovering the Distance,
Magnitude, &c. of the Fixed Stars, in consequence of the Diminution of the
Velocity of their Light."   The author suggests that by the use of a prism " we
might be able to discover diminutions in the velocity of light as perhaps a hun-
dredth, a two hundredth, a five hundredth, or even a thousandth part of the
whole."   But he then goes on to reason on the production of this diminished ve-
locity by the attraction produced on the material particles of light by the matter
of the stars, and that the diminutions stated above would be " occasioned by
spheres whose diameter should be to the sun, provided they were of the same
density, in the several proportions of 70, 50, 30, and 22 to 1 respectively."

means of apparatus now being constructed, to be able to get more accurate determinations of the velocity of the motions.

Sirius.—The comparison of the line at F with the corresponding line of hydrogen was made on several nights from January 18 to March 5. Spectroscope C and eyepieces 2 and 3 were used. These observations confirm the conclusion arrived at in my former paper, that the star is moving from the earth ; but they ascribe to the star a velocity smaller than that which I have obtained.

These observations on different days show a change of refrangibility corresponding to a velocity of from 26 miles to 36 miles per second. The part of the earth's orbital motion from the star varied on these days from 10 miles to 14 miles per second. We may take, therefore, 18 to 22 miles per second as due to the star.

The difference of this estimate, which is probably below rather than in excess of the true amount, from that which I formerly made may be due in part or entirely to the less perfect instruments then at my command. At the same time, if Sirius be moving in an elliptic orbit, as suggested by Dr. Peters, that part of the star's proper motion which is in the direction of the visual ray would constantly vary[*].

Betelgeux (α *Orionis*).—In the early observations of Dr. Miller and myself on this star, we found that there are no strong lines coincident with the hydrogen lines at C and F. The line H α falls on the less refrangible side of a small group of strong lines, and H β occurs in the space between two groups of strong lines where the lines are faint. On one night of unusual steadiness of the air, when the finer lines in the star's spectrum were seen with more than ordinary distinctness, I was able with the more powerful instruments now at my command to see a narrow defined line in the red apparently coincident with H α, and a similar line at the position of H β. These lines are much less intense than the lines C and F in the solar spectrum ; there are certainly no bright lines in the star's spectrum at these places.

The most suitable lines in this star for comparison with terrestrial substances for ascertaining the star's motion are the lines of sodium and of magnesium. The double character of the one line agreeing exactly with that of sodium, and the further circumstance that the more refrangible of the lines is the stronger one, as is the case in spectrum of sodium and in the solar spectrum, and the relative distances from each other and comparative brightness of the three lines, which correspond precisely to the triple group of magnesium, can allow of no doubt that these lines in the star are really produced by

[*] Dr. H. Vogel at Bothkamp seems to have repeated my observations on Sirius with the necessary care. He says (Astron. Nachr. No. 1864):—" Mit der eben beschriebenen Anordnung gelang es Herrn Dr. Lohse und mir am 22. März (1871) bei ganz vorzüglicher Luft die Nichtcoincidenz der drei Wasserstofflinien H α, H β, und H γ, der Geissler'schen Röhre mit den entsprechenden Linien des Siriusspectrum zu sehen ...... mit Berücksichtigung der Geschwindigkeit der Erde zur Zeit der Beobachtung berechnet sich die Geschwindigkeit mit welcher sich Sirius von der Erde bewegt zu 10·0 Meilen in der Secunde, wogegen Procyon sich 13·8 Meilen in der Secunde von unserer Erde entfernen würde."

the vapours of these substances existing there, and that we may therefore safely take any small displacement of either set of lines to show a motion of the star towards or from the earth. The lines due to sodium are perhaps more intense, but are as narrow and defined as the lines D_1, D_2 in the solar spectrum : they fall, however, within a group of very fine lines ; this circumstance may possibly account for the nebulous character which has been assigned to them by some observers.

The bright lines of sodium were compared with spectroscope B and eyepiece 3 ; they appeared to fall very slightly above the pair in the star, showing that the stellar lines have been degraded by the star's motion from the earth. The amount of displacement was estimated at about one fifth of the distance of D_1 from D_2, which is probably rather smaller than the true amount. This estimation would give a velocity of separation of 37 miles per second. At the time of observation the earth was moving from the star at about 15 miles per second, leaving 29 miles to be due to the star.

When magnesium was compared, a shift in the same direction, and corresponding in extent to about the same velocity of recession, was observed ; but in consequence of other lines in the star at this place, the former estimation, based on the displacement of the lines of sodium, was considered to be more satisfactory.

Rigel.—The lines of hydrogen are strong in the spectrum of this star, and are suitable for comparison.

The line $H\beta$ is not so broad as it appears in the spectrum of Sirius, but is stronger than F in the solar spectrum : this line was compared by means of spectroscope C and eyepieces 2 and 3. The line of terrestrial hydrogen falls above the middle of the line in the star ; the star is therefore receding from the earth. The velocity of recession may be estimated as rather smaller than Sirius, probably about 30 miles per second, the earth at the time of observation moving from the star with a velocity of 15 miles, leaving about 15 miles as due to the star. This estimate is probably rather smaller than the true velocity of the star.

Castor.—The spectra of the two component stars of this double star blend in the spectroscope into one spectrum. The line $H\beta$ is rather broad, nearly as much so as the same line in the spectrum of Sirius.

The narrow line of rarefied hydrogen was compared in spectroscope B with eyepiece 3 ; it appeared to fall on the more refrangible side of the middle of the line in the star, leaving more of the dark line on the side towards the red. The shift seemed to be rather greater than that in Sirius, and may probably be taken at from 40 to 45 miles per second ; but the earth's orbital motion was nearly 17 miles from the star, thus leaving about 25 miles for the apparent velocity of the star. This result rests at present on observations on one night only, but they seemed at the time to be satisfactory.

Regulus.—The line at F rather broad. The corresponding line of hydrogen falls on the more refrangible side of the middle of the dark

line in the star. The air was unfavourable on all the evenings of comparison ; a rough estimate gives a velocity of from 30 to 35 per second. The earth's motion was 18 miles, leaving from 12 to 17 miles for the velocity of recession between the star and the sun.

β and δ Leonis.—These stars were compared with hydrogen ; they appear to be moving from the earth, but the want of steadiness in the air prevented me from making a satisfactory estimate of their velocity. I suspected their motion to be rather smaller than that of Regulus.

β, γ, δ, ε, ζ Ursæ majoris.—All these stars have similar spectra, in which the line F is strong, though there are small differences in the breadth of the line. They were compared with hydrogen, and appear to be moving from our system with about the same velocity. Probably their motion may be taken to be not far from 30 miles per second. The earth's motion at the time of observation was from 9 miles to 13 miles from these stars, leaving a probable velocity of recession of 17 to 29 miles per second. In the case of the double star ζ, the spectrum consisted of the light of both stars.

η Ursæ majoris was also compared with hydrogen. I believe it shows a motion from the earth ; but the observations of this star are at present less satisfactory.

α Virginis and α Coronæ borealis.—These stars were compared with hydrogen. I suspect that they are receding, but I have not had nights sufficiently fine to enable me to make satisfactory observations of these stars.

In addition to these stars some observations (which are less satisfactory on account of the unfavourable state of the weather at the time) appear to show that the stars Procyon, Capella, and possibly Aldebaran are moving from the earth.

The stars which follow have a motion of approach.

Arcturus.—In the spectrum of this star the lines of hydrogen, of magnesium, and of sodium are sufficiently distinct for comparison. I found the comparison could be most satisfactorily made with magnesium.

The bright lines of magnesium fall on the less refrangible side of the corresponding dark lines in the star's spectrum, showing that the star is approaching the earth. I estimated the shift at about $\frac{1}{5}$ to $\frac{1}{4}$ of the interval between Mg_2 and Mg_3 ; this amount of displacement would indicate a velocity of approach of 50 miles per second. To this velocity must be added the earth's orbital motion from the star of 5·25 miles per second, increasing the star's motion to 55 miles per second.

When I can get favourable weather, I hope to obtain independent estimations from the lines of sodium and of hydrogen.

α Lyræ.—In the spectrum of Vega the line corresponding to H β is strong and broad. Comparisons were made on several nights, but on one evening only was the air favourable. The observations are accordant in showing that the narrow bright line from a Geissler's tube falls on the less refrangible side of the middle of the line in the star, thus leaving more of the line on the side towards the violet. The estimations give a motion of approach between the earth and

the star of from 40 to 50 miles per second, to which must be added 3·9 miles for the earth's motion from the star.

a Cygni.—The hydrogen line at F in the spectrum of this star is narrower than in the spectrum of Sirius and of α Lyræ, though probably rather broader than the same line in the solar spectrum. I have at present observations made on two evenings only, on both of which the state of the air was unfavourable for the comparison of this line with that of terrestrial hydrogen. They give to the star a motion of approach of about 30 miles per second, which would have to be increased by 9 miles, the velocity at the time of the earth from the star.

Pollux.—The lines of magnesium and those of sodium are very distinct in the spectrum of this star. As the air was not very steady at the time of my observations, I found it more satisfactory to use for comparison the lines of magnesium, which are rather stronger than those of sodium. The three lines of magnesium appeared to be less refrangible than the corresponding dark lines in the spectrum of the star by about one-sixth of the interval from Mg_2 to Mg_3. This estimation would represent a velocity of approach equal to about 32 miles per second. The earth's motion from the star was 17·5 miles, which increases the apparent velocity of approach to 49 miles per second. On one evening only was the air favourable enough for a numerical estimate, but the observations were entered in my observatory-book as satisfactory.

a Ursæ majoris.—The spectrum of this star is different from the spectra of the other bright stars of this constellation. The line at F is not so strong, while the lines at *b* are more distinct, and are sufficiently strong for comparison with the bright lines of magnesium. The bright lines of this metal fall on the less refrangible side of the dark lines, and show a motion of approach of from 35 to 50 miles per second. The earth's motion of 11·8 miles from the star must be added.

γ Leonis and ε Boötis.—In both these double stars the compound spectrum due to the light of both component stars was observed. Both stars are most conveniently compared with magnesium. I do not consider my observations of these stars as quite satisfactory, but they seem to show a movement of approach; but further observations are desirable.

The stars *γ Cygni, α Pegasi, γ Pegasi,* and *α Andromedæ* were compared with hydrogen on one night only. It is probable that these stars are approaching the earth, but I wish to reobserve them before any numerical estimate is given of their motion.

γ Cassiopeiæ.—On two nights I compared the bright lines which are present in its spectrum at C and F with the bright lines of terrestrial hydrogen. The coincidence appeared nearly perfect in spectroscope C with eyepieces 2 and 3; but on the night of best definition I suspected a minute displacement of the bright line towards the red when compared with H β. As the earth's orbital motion from the star at the time was very small, about 3·25 miles per second, which corresponds to a shift that could not be detected in the spectroscope,

it seems probable that γ Cassiopeiæ has a small motion of recession.

In the calculation of the estimated velocities the wave-lengths employed are those given by Ångström in his ' Recherches sur le spectre solaire' (Upsal, 1868). The velocity of light was taken at 185,000 miles per second.

The velocities of approach and of recession which have been assigned to the stars in this paper represent the whole of the motion in the line of sight which exists between them and the sun.   As we know that the sun is moving in space, a certain part of these observed velocities must be due to the solar motion.   I have not attempted to make this correction, because, though the direction of the sun's motion seems to be satisfactorily ascertained, any estimate that can be made at present of the actual velocity with which he is advancing must rest upon suppositions, more or less arbitrary, of the average distance of stars of different magnitudes.   It seems not improbable that this part of the stars' motions may be larger than would result from Otto Struve's calculations, which give, on the supposition that the average parallax of a star of the first magnitude is equal to $0''\cdot209$, a velocity but little greater than one fourth of the earth's annual motion in its orbit.

It will be observed that, speaking generally, the stars which the spectroscope shows to be moving from the earth (Sirius, Betelgeux, Rigel, Procyon) are situated in a part of the heavens opposite to Hercules, towards which the sun is advancing, while the stars in the neighbourhood of this region, as Arcturus, Vega, α Cygni, show a motion of approach.   There are in the stars already observed exceptions to this general statement ; and there are some other considerations which appear to show that the sun's motion in space is not the only, or even in all cases, as it may be found, the chief cause of the observed proper motions of the stars[*].

There can be little doubt that in the observed stellar movements we have to do with two other independent motions—namely, a movement common to certain groups of stars, and also a motion peculiar to each star.

Mr. Proctor has brought to light strong evidence in favour of the drift of stars in groups having a community of motion, by his graphical investigation of the proper motions of all the stars in the catalogues of Mr. Main and Mr. Stone[†]. The probability of the stars being collected into systems was early suggested by Michell and the elder Herschel[‡]. One of the most remarkable instances

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[*] As the velocities assigned to the stars are, for reasons already stated, provisional only, I feel some hesitation in drawing from them the obvious conclusions which they would suggest. The velocities given in the Tables for those stars which are moving in direction in accordance with the sun's motion towards Hercules do not bear to each other the relation which they should have if they were mainly produced by the sun's motion. Even for these stars, therefore, we must look elsewhere for the cause to which they are chiefly due.

[†] See " Preliminary Paper on certain Drifting Motions of the Stars," Proc. Roy. Soc. vol. xviii. p. 169.

[‡] Sir William Herschel writes :—" Mr. Michell's admirable idea of the stars

pointed out by Mr. Proctor are the stars β, γ, δ, ϵ, ζ of the Great Bear, which have a community of proper motions*, while α and η of the same constellation have a proper motion in the opposite direction. Now, the spectroscopic observations show that the stars β, γ, δ, ϵ, ζ have also a common motion of recession, while the star α is approaching the earth. The star η, indeed, appears to be moving from us, but it is too far from α to be regarded as a companion to that star.

Although it was not to be expected that a concurrence would always be found between the proper motions which indicate the apparent motions at right angles to the line of sight and the radial motions as discovered by the spectroscope, still it is interesting to remark that in the case of the stars Castor and Pollux, one of which is approaching and the other receding, their proper motions also are different in direction and in amount—and further, that γ Leonis, which has an opposite radial motion to α and β of the same constellation, differs from these stars in the direction of its proper motion.

TABLE I.—Stars moving from Sun.

Star.	Compared with	Apparent motion.	Earth's motion.	Motion from sun.
Sirius.............................	H	28 to 36	−10 to 14	18 to 22
Betelgeux	Na	37	−15	22
Rigel	H	30	−15	15
Castor	H	40 to 45	−17	23 to 28
Regulus...........................	H	30 to 35	−18	12 to 17
β Ursæ majoris	H			
γ ,, ,, ...	H			
δ ,, ,, ...	H	30	− 9 to 13	17 to 21
ϵ ,, ,, ...	H			
ζ ,, ,, ...	H			
β Leonis	H			
δ Leonis	H			
η Ursæ majoris	H			
α Virginis	H			
α Coronæ borealis	H			
Procyon	H			
Capella	H			
Aldebaran ?	Mg			
γ Cassiopeiæ..................	H			

being collected into systems appears to be extremely well founded, and is every day more confirmed by observations, though this does not take away the probability of many stars being still as it were solitary, or, if I may use the expression, intersystematical ...... A star, or sun such as ours, may have a proper motion within its own system of stars, while at the same time the whole starry system to which it belongs may have another proper motion totally different in quantity and direction." Herschel further says, " And should there be found in any particular part of the heavens a concurrence of proper motions of quite a different direction, we shall then begin to form some conjectures which stars may possibly belong to ours, and which to other systems."—Phil. Trans. 1783, pp. 276, 277.

* Mr. Proctor, speaking of these stars, says :—"Their drift is, I think, most significant. If, in truth, the parallelism and equality of motion are to be regarded as accidental, the coincidence is one of most remarkable character. But such an interpretation can hardly be looked upon as admissible when we remember that the peculiarity is only one of a series of instances, some of which are scarcely less striking."—Other Worlds than Ours, p. 269. See paper in Proc. Roy. Soc. vol. xviii. p. 170.

It scarcely needs remark that the difference in breadth of the line H β in different stars affords us information of the difference of density of the gas by which the lines of absorption are produced. A discussion of the observations in reference to this point, and other considerations on the physical condition of the stars and nebulæ, I prefer to reserve for the present.

TABLE II.—Stars approaching the Sun.

Star.	Compared with	Apparent motion.	Earth's motion.	Motion towards sun.
Arcturus	Mg	50	+ 5	55
Vega	H	40 to 50	+ 3·9	44 to 54
α Cygni.........................	H	30	+ 9	39
Pollux	Mg	32	+17	49
α Ursæ majoris...............	Mg	35 to 50	+11	46 to 60
γ Leonis	Mg			
ε Boötis.........................	Mg			
γ Cygni.........................	H			
α Pegasi.........................	H			
γ Pegasi?	H			
α Andromedæ	H			

December 12.—William Spottiswoode, M.A., Treasurer and Vice-President, in the Chair.

The following communication was read :—

" Researches in Spectrum Analysis in connexion with the Spectrum of the Sun."—No. I. By J. Norman Lockyer, F.R.S.

The author, after referring to the researches in which he has been engaged since January 1869 in conjunction with Dr. Frankland, refers to the evidence obtained by them as to the thickening and thinning of spectral lines by variations of pressure, and to the disappearance of certain lines when the method employed by them since 1869 is used. This method consists of throwing an image of the light-source to be examined on to the slit of the spectroscope.

It is pointed out that the phenomena observed are of the same nature as those already described by Stokes, W. A. Miller, Robinson, and Thalen, but that the application of this method enables them to be better studied, the metallic spectra being clearly separated from that of the gaseous medium through which the spark passes. Photographs of the spark, taken in air between zinc and cadmium and zinc and tin, accompany the paper, showing that when spectra of the vapours given off by electrodes are studied in this manner, the vapours close to the electrode give lines which disappear from the spectrum of the vapour at a greater distance from the electrode, so that there appear to be long and short lines in the spectrum.

The following elements have been mapped on this method :— Na, Li, Mg, Al, Mn, Co, Ni, Zn, Sr, Cd, Sn, Sb, Ba, and Pb, the lines being laid down from Thalen's maps, and the various characters and lengths of the lines shown.

In some cases the spectra of the metals, enclosed in tubes and sub-

jected to a continually decreasing pressure, have been observed. In all these experiments the lines gradually disappear as the pressure is reduced, the *shortest lines disappearing first, and the longest lines remaining longest visible.*

Since it appeared that the purest and densest vapour alone gave the greatest number of lines, it became of interest to examine the spectra of compounds consisting of a metal combined with a non-metallic element. Experiments with chlorides are recorded. It was found in all cases that the difference between the spectrum of the chloride and the spectrum of the metal was that under the same spark-conditions all the short lines were obliterated. Changing the spark-conditions, the final result was that only the very longest lines in the spectrum of the metallic vapour remained. It was observed that in the case of elements with low atomic weights, combined with one equivalent of chlorine, the numbers of lines which remain in the chloride is large, 60 per cent., *e. g.*, in the case of Li and 40 per cent. in the case of Na; while in the case of elements with greater atomic weights, combined with two equivalents of chlorine, a much smaller number of lines remain—8 per cent. in the case of barium, and 3 per cent. in the case of Pb.

The application of these observations to the solar spectrum, to elucidate which they were undertaken, is then given.

It is well known that all the known lines of the metallic elements on the solar atmosphere are not reversed. Mr. Lockyer states what Kirchhoff and Ångström have written on this subject, and what substances, according to each, exist in the solar atmosphere. He next announces the discovery that, with no exception whatever, *the lines which are reversed are the longest lines.* With this additional key he does not hesitate to add, on the strength of a small number of lines reversed, zinc and aluminium (and possibly strontium) to the last list of solar elements given by Thalen, who rejected zinc from Kirchhoff's list, and agreed with him in rejecting aluminium. It need scarcely be added that these lines are in each case the longest lines in the spectrum of the metal.

The help which these determinations afford to the study of the various cyclical changes in the solar spectra is then referred to.

GEOLOGICAL SOCIETY.

[Continued from vol. xliv. p. 543.]

May 22, 1872.—Prof. Morris, V.P., in the Chair.

The following communication was read :—

" Some observations on the Upper Greensand Formation of Cambridge." By W. Johnston Sollas, Esq., Assoc. Royal School of Mines, London.

The author supposes that the coprolites are in all cases the result of the fossilization of organic matter, or of the intermediate products of its decomposition—a large number being simply fossil sponges, and the rest merely the phosphatization of animal matter

so far decomposed as to have lost almost every trace of its original structure.   He said that it had been suggested that the decomposition of small fish must have furnished the Gault with a large proportion of that phosphatic matter which saturated the last-deposited clay of the formation, and which served to fossilize the decaying organisms imbedded in it.   He supposes that the roughly cylindrical forms having a core of white chalk-marl with an outer annular portion of coprolitic material are sponges.   He said that he had obtained from the calcareous portions of this deposit numerous specimens of Foraminifera belonging to genera of which he gave a list.   He thought that the green grains are casts of Foraminifera. He stated that he has obtained several glauconitic casts still coated with the shells of the Foraminifera in which they were formed.   In conclusion the author pointed out the unreliable nature of the analyses made of the green grains.

June 5, 1872.—J. Gwyn Jeffreys, Esq., F.R.S., in the Chair.

The following communications were read:—

1. " Notes on Sand-pits, Mud-volcanoes, and Brine-pits met with during the Yarkand Expedition of 1870." By George Henderson M.D., F.L.S.

The author described some very remarkable circular pits which occurred chiefly in the valley of the Karakash river.   These pits varied in diameter from 6 to 8 feet, and were between 2 and 3 feet deep, the distances between the pits being about the same as the diameters.   He accounted for the formation of the pits by supposing that the water, which sinks into the gravel at the head of the valley, flows under a stratum of clay, which prevents it from rising ; the water in course of time, however, flowing in very varying quantities at different periods, gradually washes away small portions of the clayey band, when the sand above runs through into the cavity thus formed, leaving the pits described by the author.   The mud-volcanoes at Tarl Dab he accounted for by supposing that after a fall of rain or snow the air contained in the water-bearing stratum would get churned up with water and mud, and be ejected as a frothy mud, sometimes to a height of 3 feet ; while the brine-pits in the Karakash valley he believed to be formed by the excessive rise and fall in the level of that river at various times, which alternately fills and empties the bottoms of the pits, and the water left in the pits gets gradually concentrated by evaporation until a strong brine remains.

2. " On the Cervidæ of the Forest-bed of Norfolk and Suffolk." By W. Boyd Dawkins, Esq., M.A., F.R.S., F.G.S.

The author described a new form of *Cervus* from the Forest-bed of Norfolk, which he based on a series of antlers, and named *C. verticornis*.   The base of the antler is set on the head very obliquely ; immediately above it springs the cylindrical brow-tyne, which suddenly curves downwards and inwards ; immediately above the brow-tyne the beam is more or less cylindrical, becoming gradually

flattened. A third, flattened tyne springs on the anterior side of the beam; and immediately above it the broad crown terminates in two or more points. No tyne is thrown off on the posterior side of the antler, and the sweep is uninterrupted from the antler-base to the first point of the crown. The antlers differ in curvature and otherwise from those of *Cervus megaceros*; but there is a general resemblance between the two animals, and the *verticornis* must have rivalled the Irish Elk in size. A second species of Deer, the *Cervus carnutorum*, which had been furnished by the strata of St.-Prest near Chartres, must be added to the fauna of the Forest-bed. The Cervidæ of the Forest-bed present a remarkable mixture of forms, such as the *Cervus polignacus*, *C. Sedgwickii*, *C. megaceros*, *C. carnutorum*, *C. elaphus*, and *C. capreolus*, seeming to indicate that in classification the Forest-bed belongs rather to an early stage of the Pleistocene than to the Pliocene age. This inference is strongly corroborated by the presence of the Mammoth, which is so characteristic of the Pleistocene age.

3. " The Classification of the Pleistocene Strata of Britain and the Continent by means of the Mammalia." By W. Boyd Dawkins, Esq., M.A., F.R.S., F.G.S.

The Pleistocene deposits may be divided into three groups :—1st, that in which the Pleistocene immigrants lived, with some of the southern and Pliocene animals in Britain, France, and Germany, and in which no arctic mammalia had arrived; 2nd, that in which the characteristic Pliocene Cervidæ had disappeared, and the *Elephas meridionalis* and *Rhinoceros etruscus* had been driven south; 3rd, that in which the true arctic mammalia were the chief inhabitants.

This third or late Pleistocene division must be far older than any Prehistoric deposits, as the latter often rest on the former, and are composed of different materials; but the difference offered by the fauna is the most striking. In the Pleistocene river-deposits twenty-eight species have been found, the remains of man being associated with the Lion, Hippopotamus, Mammoth, Wolf, and Reindeer. On examining the fauna from the ossiferous caves, we find the same group of animals, with the exception of the Musk-sheep; and it is therefore evident that the cave-fauna is identical with that of the river strata, and must be referred to the same period. Some few animals, however, which would naturally haunt caves, are peculiar to them, as the Cave-bear, Wild Cat, Leopard, &c.

The magnitude of the break in time between the Prehistoric and late Pleistocene period may be gathered also from the disappearance in the interval of no less than nineteen species.

The middle division of the Pleistocene mammalia, or that from which the Pliocene Cervidæ had disappeared and been replaced by invading temperate forms, is represented in Great Britain by the deposits of the Lower Brick-earths of the Thames valley, and the older deposits in Kent's Hole and Oreston. The discovery, by the Rev. O. Fisher, of a flint-flake in the undisturbed Lower Brick-earth at Crayford proves that man must have been living at this time.

The mammalia from these deposits are linked to the Pliocene by the *Rh. megarhinus*, and to the late Pleistocene by the *Ovibos moschatus*. The presence of *Machærodus latidens* in Kent's Hole, and of the *Rh. megarhinus* in the cave at Oreston, tends to the conclusion that some of the caves in the south of England contain a fauna that was living before the late Pleistocene age. The whole assemblage of middle Pleistocene animals evinces a less severe climate than in the late Pleistocene times.

The fossil bones from the Forest-bed of Norfolk and Suffolk show that in the early Pleistocene mammalia there was a great mixture of Pleistocene and Pliocene species. It is probable also that the period was one of long duration; for in it we find two animals which are unknown on the continent, implying that the lapse of time was sufficiently great to allow of the evolution of forms of animal life hitherto unknown, and which disappeared before the middle and late Pleistocene stages.

The author criticised M. Lartet's classification of the Late Pleistocene or Quaternary period by means of the Cave-bear, Mammoth, Reindeer, and Aurochs, and urged that, since the remains of all these animals were intimately associated in the caves of France, Germany, and Britain, and, so far as we know, the first two appeared and disappeared together and the last two lived on into the Prehistoric age, they did not afford a basis for a chronology.

The latest of the three divisions of the British Pleistocene fauna is widely spread through France, Germany, and Russia, from the English Channel to the shores of the Mediterranean. The Middle Pleistocene is represented by a river-deposit in Auvergne, and by a cave in the Jura, in which the presence of the *Machærodus latidens*, and a non-tichorine Rhinoceros, and the absence of the characteristic arctic group of the late Pleistocene and of all the peculiar animals of the early Forest-bed stage, prove that that era must be Middle Pleistocene. The Early Pleistocene division is represented in France by the river-deposit at Chartres, being characterized by the presence of two non-Pliocine animals, *Trogontherium* and *Cervus carnutorum*.

The Pleistocene mammalia of the regions south of the Alps and Pyrenees present no trace of truly arctic species, the Mammoth being viewed as an animal fitted for the climatal conditions both of Northern Siberia and of the southern states of America. It contains *Elephas africanus* and *Hyæna striata*.

The fauna of Sicily, Malta, and Crete differs considerably from that described above, possessing some peculiar forms, such as *Hippopotamus Pentlandi*, *Myoxus melitensis* and *Elephas melitensis*.

The Pleistocene mammalia may be divided into five groups, each marking a difference in the climate:—the first embracing those which now live in hot countries; the second those which inhabit northern regions, or high mountains, where the cold is severe; the third those which inhabit temperate regions; a fourth those which are found alike in hot and cold; and a fifth, which are extinct.

There were three climatal zones, marked by the varying range of

the animals:—the northern, into which the southern forms never penetrated, the latitude of Yorkshire being the boundary of the advance of the southern animals; the southern, into which the northern species never passed, a line passing through the Alps and Pyrenees being the limit of the range of the northern animals; and an intermediate area, in which the two are found mingled together.

Two out of the three zones are proved by the physical evidence of the Pleistocene strata.

We see by the discoveries of Dr. Bryce, Mr. Jameson, and others, that the Pleistocene mammalia must have invaded Europe during the first Glacial period before the submergence; for the Reindeer and the Mammoth have been found in Scotland under the deposits of the Boulder-clay. Dr. Falconer and others have also discovered the latter animal in the preg'acial Forest-bed. The Glacial period can therefore no longer be looked on as a hard and fast barrier separating one fauna from another. If man be treated as a Pleistocene animal, there is reason to believe that he formed one of the North Asiatic group, which was certainly in possession of Northern and Central Europe in Preglacial times.

The Pleistocene mammalia may again be divided into three groups—those which came from Northern and Central Asia, those fro n Africa, and those which were living in the same area in the Pliocene age. Had not the animals which lived in Europe, during the Pliocene age, been insulated from those which invaded Europe from A ia, by some impassable barrier, the latter would occur in our Pliocene strata as well as the former. Such a barrier is offered by the northern extension of the Caspian up the valley of the Obi to the Arctic Sea. The animals of Northern and Central Asia could not pass westwards u til the barrier was removed by the elevation of the sea-bottom between the Caspian and the Urals.

The same argument holds good as to the African mammalia, which could not have passed into Sicily, Spain, or Britain without a northward extension of the African mainland.

The relation of the Pleistocene to the Pliocene fauna is a question of great difficulty. If the Pliocene fauna be compared with that of the Forest-bed, it will be seen that the difference between them is very great. The Pliocene Mastodon a id Tapir, and most of the Cervidæ, are replaced by forms such as the Roe and Red Deer, unknown until then; but many of the Pliocene animals were able to hold their ground against the Pleistocene invaders, although they were ultimately beaten in the struggle for existence by the new comers. The fauna which the author adopted as typically Pliocene is that furnished by the lacustrine strata of Auvergne, the marine sands of Montpellier, and the older fluviatile strata of the Val d'Arno.

XIX. *Intelligence and Miscellaneous Articles.*

THE INVENTION OF THE WATER AIR-PUMP. BY H. SPRENGEL.

[*Statement**.

A LETTER addressed to me by Dr. Sprengel under date of No-
vember 1st, 1872, in which he says, " Perhaps it will not have
escaped your observation, that the invention of the water air-pump,
which you have constructed after the principle of my mercury air-
pump, according to your paper published in 1868 on the Washing
of Precipitates, is almost everywhere attributed to you," induces
me to make the following statement.

The interesting discovery, that by means of columns of liquids flow-
ing downwards a more perfect vacuum can be produced than was pos-
sible by the air-pumps hitherto in use, belongs solely and only to
Dr. Sprengel. He in his researches on the Vacuum (Journal of
the Chemical Society, January 1865), brings prominently forwards
that water is, from a practical point of view, the only liquid which
could come into consideration as a substitute for mercury, used in
the instrument described by him, and that it is not unlikely that
such an instrument, adapted for water, might possess advantages
which air-pumps of other constructions have not, particularly in
hilly countries, where the large volume of a natural waterfall might
be rendered available. In the theoretical considerations on the
action of his instrument, which immediately follow the above, it is
noticed that it is simply the reverse of the trompe, with this addi-
tion, that the supply of air is limited, while that in the trompe is
unlimited.

If in the face of these facts, which are open to all, any one attri-
butes to me, as I must conclude from Dr. Sprengel's letter, a share
in his discovery, I can regret this only all the more keenly, as in
my treatise on the new method of filtration I could not possibly
have expressed myself with regard to Dr. Sprengel's claims more
loyally and precisely than I have done. There I have stated ex-
pressly that I have constructed the pump, used for filtrations and
described by me in detail, after the principle of Sprengel's mercury
air-pump. It was the only apparatus of the kind which Dr. Sprengel
described, consequently the one to which alone I could refer.

Heidelberg, November 5, 1872.   (Signed)   R. BUNSEN.]

Expressing my best thanks to Professor Bunsen for the above
statement, I beg to add that since 1860 I have been using for labo-
ratory purposes a water-trompe, as described by me in Poggendorff's
Annalen for 1861 (vol. cxii.), which (by reversing the action) led
me in 1863 to the new method of air-rarefaction. Water was the
first liquid which I used in my first pump, constructed during the

* Translated from *Ann. Chem. Pharm.* vol. clxv. p. 159, by H. Sprengel,
authorized by Professor Bunsen.

summer of 1863. But the fallacies arising from the tension of aqueous vapour and from the air absorbed in water, as well as the inconvenience of having to provide for the requisite fall, caused me to discontinue the use of water, and to substitute in its stead mercury as the most suitable liquid for establishing *the* truth, which I had recognized by means of a water air-pump with an insufficient fall. My paper of 1865 was written with reference to *all* liquids ; in fact on p. 15 (rendered prominent by italics) I summed up thus :—

"The main fact which I have established in this paper may be shortly stated to be, *that if a liquid be allowed to run down a tube, to the upper part of which a receiver is attached by means of a lateral tube, and if the height at which the receiver is attached be not less than that of the column of the liquid which can be supported by the atmospheric pressure, a vacuum will be formed in the receiver*, minus *the tension of the liquid employed*."

I regret that the obviousness of the matter led me to refrain from expressing myself in a more detailed manner, believing, as I still believe, that what I wrote sufficiently described the construction of the water air-pump.

In conclusion, Mr. Johnson's aspirator * for establishing a current of air ought to be mentioned here. It was recognized by Professor Hofmann † to act on the principle of the trompe, and, of course, might have served as an air-pump, had it been noticed at the time that the instrument would furnish the means of creating a vacuum. And I may also draw attention to the tube ‡ of a vacuum-pan, through which the water is made to escape, which has served to condense the steam of the boiling liquid. This no doubt would in like manner have served as a complete water air-pump ; but it does not appear that its use as such was discovered.

London, January 22, 1873.

REPORT ON THE RESEARCHES OF M. ARN. THENARD CONCERNING THE ACTIONS OF ELECTRIC DISCHARGES UPON GASES AND VAPOURS. BY EDM. BECQUEREL.

The publications of M. Arnould Thenard which the Academy commissioned us to examine, have reference to the decomposition effects produced by electric discharges on gases and vapours, especially carbonic acid.

The effects due to the action of the electric spark on compound gases are very complex ; for if, on the one hand, decomposition may take place, on the other the separated elements, if they remain gaseous, tend to reconstitute the primitive compound. The

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* Quarterly Journal of the Chemical Society, vol. iv. p. 186 (1852).
† *Vide* the same paper.
‡ Elements of Physics, by Neil Arnott, M.D. Longmans. 3rd edition. London, 1828.

final result, therefore, after an action of a certain duration, must differ according to whether one of the elements separated is solid, liquid, or gaseous at the surrounding temperature, and must depend on the more or less elevated temperature produced by the passage of the spark, as well as the recompositions which may be effected in the vicinity of the latter. M. Thenard placed himself in conditions such that the calorific action extended only the least distance possible around the electrified points. Instead of sparks bursting forth in a eudiometric tube, he made use of the electrical effluvium —that is to say, more or less obscure discharges produced from place to place among the gaseous particles themselves. For this purpose he had recourse to the simple and ingenious arrangement of apparatus devised by M. Houzeau for the production of ozone, as the conditions necessary to that allotropic transformation of oxygen appeared similar to those which he proposed to utilize. This arrangement permitted him, besides, to submit to the electric influence successively and in distinct portions any volumes whatever of gas or vapour.

Several important additions to and modifications of this mode of experimentation have been made by M. Arnould Thenard, and very carefully studied for the purpose of ascertaining the most favourable conditions for the production of ozone as well as for the decomposition of carbonic acid. His observations led him to perceive that it is preferable to produce the electric effluvium between smooth surfaces of glass instead of between metallic conductors. He likewise saw that the action of electricity disaggregates glass at its surface, covering it with a fine powder which ends by transforming little by little the effluvium into sparks—that is to say, gives to the discharge a form which not only does not produce the effects of the effluvium, but may even destroy them. By removing this powder the efficacious action of the smooth tubes is reestablished. In certain circumstances which he indicates, electrochemical deposits in the tubes may give rise to the same effects.

His researches relate particularly to carbonic acid, the partial decomposition of which has, from the end of the last century, been the subject of several investigations on account of the action of the spark upon this gas being opposite to its action upon a mixture of carbonic oxide and oxygen, the two latter gases being capable of reconstituting carbonic acid in a eudiometer. He has ascertained that, with a very gentle current of carbonic acid circulating in the special apparatus he makes use of, decomposition into carbonic oxide and oxygen may reach 26·5 per cent. of its volume ; while, as De Saussure observed, if we operate by means of sparks, it does not exceed 7·5 per cent.

He likewise shows that the preceding mixtures, containing as much as 26·5 per cent. of decomposed carbonic acid, revert in the gaseous state to 7·5 per cent. in the eudiometer—the greatest elevation of temperature due to the sparks in the latter experimental conditions doubtless not rendering possible, as shown by M. Berthelot's experiments, an explosive mixture of carbonic oxide and

oxygen in greater proportions than these. We will mention also that the oxygen proceeding from the decomposition of carbonic acid in the apparatus in question was sensibly ozonized. It must be remarked that the decomposition of carbonic acid by electricity in these circumstances is effected at apparently a very low temperature, and that this seems to be the first time that an approximation has been made to conditions analogous to those of the decomposition of that gas by green leaves under the influence of the light of the sun.

It would be desirable that these new experiments, which have been made with much care, and the principal results of which we have been able to verify, should be extended to other gases and vapours—the electric intensities being varied between wider limits, as well as the surrounding temperature and the velocity of the gaseous currents.

Physico-chemical researches directed to the modifications caused by electricity in simple substances and in compounds present great scientific interest; for they may elucidate the question, still so obscure, of the allotropy of simple bodies, and may lead to the explanation of the decomposition undergone by certain compounds in the organism.—*Comptes Rendus de l'Acad. des Sciences*, vol. lxxv. pp. 1735–1737.

GREAT BAROMETRIC DEPRESSION OF JANUARY.

On the 16th of January, 1873, a fall of the barometer of no ordinary magnitude set in at several stations in the north-west of Europe. This fall became more general on the 17th, was fully established, except at Biarritz, on the 18th, and continued with such rapidity that at sixteen stations the fall exceeded 1 inch of mercury between 8 A.M. of the 18th and 8 A.M. of the 19th. In the evening of this day a violent gale from the south-west accompanied this rapid fall. Except at a few stations in France, the minimum reading (below 29 inches) occurred on the 20th. The following readings at Thurso, in the north of Scotland, will show the extent of the entire fall; while those at Toulon will indicate the extensive area over which the fall occurred.

Thurso.			Toulon.		
	in.			in.	
Jan. 16.	29·89	0·42	Jan. 16.	30·36	0·16
17.	29·47	0·39	17.	30·20	0·17
18.	29·08	0·95*	18.	30·03	0·08
19.	28·13	0·02	19.	29·95	0·60*
20.	28·11		20.	29·35	0·05
			21.	29·30	
Total fall	.... 1·78		Total fall	.... 1·06	

The corresponding portion of the great fall occurred a day later at Toulon than at Thurso, as shown by the asterisks.

It is noteworthy that in 1872 a somewhat similar depression occurred on January 17–18. The recorded minima were as follows :—
Kew, 28·90 ; Falmouth, 28·75 ; Valencia, 28·50 ; Aberdeen, 28·09 ;
Glasgow, 28·08 ; Stonyhurst, 28·07 ; Armagh, 28·06. In 1871 a
similar depression occurred on January 15–16, the minima being as
follows :—Kew, 28·878 ; Falmouth, 28·769 ; Stonyhurst, 28·592 ;
Aberdeen, 28·411 ; Glasgow, 28·313 ; Valencia, 28·198 ; Armagh,
28·176. In 1870 a depression occurred on January 14, minima as
follows :—Kew, 29·40 ; Falmouth, 29·30 ; Valencia, 29·15 ; Aberdeen, 29·00 ; Glasgow, 28·85 ; Stonyhurst, 28·80 ; Armagh, 28·75.
In 1869 a depression occurred on January 14–15, minima as follows :—Kew, 29·55 ; Aberdeen, 29·50 ; Glasgow, 29·25 ; Falmouth,
29·15 ; Armagh, 29·05 ; Stonyhurst, 29·05 ; Valencia, 28·85.
These depressions taking place so nearly at the same epoch in five
consecutive years, appear to indicate that about this time in January
there is a tendency to a great reduction of pressure over Western
Europe.

The above may be interesting at the present time, especially as
the last great depression is so recent.

January 24, 1873. W. R. BIRT.

ON THE THERMAL EFFECTS OF MAGNETIZATION. BY J. MOUTIER.

The experiments of MM. Jamin and Roger have shown that
the intermittent passage of a current in the wire of an electromagnet produces heat : heat is developed at the interruption of the
circuit ; it is due to the vanishing of the temporary magnetism of
the electromagnet. M. Cazin has lately announced*, after new
experiments, that the heat thus produced is *proportional to the
square of the intensity of the magnetism and to the polar distance.* I
have sought to account for this simple law by theoretical considerations.

M. Clausius† has demonstrated the following theorem relative
to the stationary motion of a system of points—that is to say, a
motion in which the position and the velocity of each point do not
continually change in one and the same direction, but remain comprised within certain limits :—*The mean vis viva of the system is
equal to its virial.* The virial, which in questions of mechanics plays
a part analogous to that of the potential, is, as is known, half the
sum of the products obtained by multiplying the distance between
any two points of the system by the force which acts between those
two points.

This theorem conducts to peculiar consequences in the case of
magnetization. Let us consider a lengthened bar of soft iron, and
suppose magnetism developed by placing the bar in the centre of
a coil which is traversed by a current.

* *Comptes Rendus de l'Acad. des Sciences,* vol. lxxv. p. 1265.
† *Comptes Rendus,* vol. lxx. p. 1314 ; Phil. Mag. S. 4. vol. xl. p. 122.

This magnet may be regarded as consisting of an infinity of magnetic elements of constant and infinitely little thickness. The quantity of magnetism Y developed in each of the elements varies with its distance x from one of the extremities of the bar, and may be represented by $Y = \phi(x)$.

The quantity of free magnetism upon the element dx of the bar is the difference of the values of Y at the points which have for abscissæ x and $x + dx$; so that the quantity of free magnetism at the point situated at the distance x from the extremity of the bar is $y = \dfrac{dY}{dx} = \phi'(x)$. The quantity of free magnetism in one point is therefore proportional to the quantity of magnetism of the bar.

Supposing the bar composed of two symmetrical parts, let us consider one of them in particular. Starting from the extremity, the free magnetism diminishes till at a certain distance λ it becomes sensibly *nil*; beyond, the function ϕ retains a sensibly constant value $\phi(\lambda)$.

The pole of this portion of the bar is the centre of a system of parallel forces proportional to the quantities of free magnetism; the distance x from the pole to the extremity of the bar is determined by the theorem of the moments,

$$ X \int_0^\lambda y\,dx = \int_0^\lambda xy\,dx. $$

If we suppose the bar sufficiently long, so that the magnetism developed at the extremity may be neglected, we find easily

$$ X\phi(\lambda) = \lambda\phi(\lambda) - \int_0^\lambda \phi(x)\,dx. $$

The magnetizing has given rise to attractive forces between the several magnetic elements, or, according to Ampère's theory, between the parallel currents circulating in the solenoid formed by the magnet. The element whose abscissa is x is solicited by forces exerted by the elements around it, forces proportional to the quantities of magnetism of the acting elements, and of which the intensity rapidly decreases in proportion as the distance augments.

The increment of the virial, relative to the point under consideration, which results from the magnetizing may be represented by $\mu\phi(x)$,—μ denoting a function of the distance, being at the same time proportional to the quantity of magnetism developed in the bar, and consequently to the free magnetism. Besides, $\phi(x)$ is a function proportional to the quantity of free magnetism of the bar; consequently every term $\mu\phi(x)$ of the virial is proportional to the square of that quantity.

Designating by l half the length of the bar, the increment of the virial which results from the magnetizing is, for the half of the bar,

$$ \int_0^l \mu\phi(x)\,dx = \int_0^\lambda \mu\phi(x)\,dx + \int_\lambda^l \mu\phi(x)\,dx. $$

Remarking that $\phi(x)$ preserves the constant value $\phi(\lambda)$ in the interval from l to λ, and also taking account of the equation which determines the position of the pole, we find, lastly,

$$\int_0^l \mu\varphi(x)dx = \mu\phi(\lambda)(l-X).$$

The increase of *vis viva* produced in the bar by the effect of the magnetizing is therefore proportional to the square of the intensity of the magnetism and to the polar distance. The effect of demagnetizing corresponds to an equal loss of *vis viva*—which is the measure of the thermal effect produced, if that effect is the only one which accompanies the demagnetization.—*Comptes Rendus de l'Acad. des Sciences*, vol. lxxv. pp. 1619–1621.

ENCKE'S COMET.

Appendix II. of the 'Washington Observations' for 1870 contains a very interesting report by Professors Hall and Harkness on observations of Encke's comet during its return in 1871. The report of Professor Hall is confined almost exclusively to the micrometrical observations for position made with the filar micrometer of the $9\frac{1}{2}$-inch equatorial of the Naval Observatory. In the closing portion Professor Hall describes the appearances of the comet from the evening it was found, October 11, to December 7. The report is accompanied by four exquisite engravings, representing the comet as seen on October 17, November 17, December 1, and December 2. A note is added on the general fact of a condensation of the matter of the comet and formation of a nucleus as it approaches to, and an expansion of the matter and a disappearance of the nucleus as it recedes from, the sun.

Professor Harkness enters elaborately into the spectroscopic observations of the comet, and describes the instruments employed. The spectrum consisted of three bright bands of the following wave-lengths : 549·5, 510·6, and 455. By a process of interpolation, Professor Harkness finds that the wave-lengths of comet II. 1868, observed by Dr. Huggins, are so nearly identical with the above as to lead to the conclusion that the physical constitutions of the two comets are identical, and that both are composed of incandescent carbon in a gaseous state.

The investigation of the density of the supposed resisting medium in space is exceedingly interesting. Professor Harkness deduces the density in terms of the height in inches of the column of mercury which it will support, and finds that it is somewhere between $\frac{220}{10^{17}}$ and $\frac{285}{10^{20}}$ of an inch, which is "enormously greater than that of the atmosphere at the upper limit of auroras." Hence the probability that auroras are propagated in a medium which pervades all

space, and that the spectrum of the aurora is in reality the spectrum of that medium.

Professor Harkness calls attention to the continual increase in the wave-length of the light emitted by the brightest part of the second band of the spectrum as a phenomenon hitherto unobserved. He refers it to an increase of the temperature of the comet as it approached the sun.

ON THE INTENSITY OF SOUND AND LIGHT.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

Glenville, Fermoy, Jan. 6, 1873.

The subject of Mr. Moon's paper (Phil. Mag. vol. xlv. p. 38) deserves consideration *physically* as well as mathematically. The formula for the intensity must necessarily embrace two factors, viz. one representing the *number* of waves (of a given length), and the other giving the *amplitude of the vibration* of the particle of æther under consideration in each particular wave.

In regard to the former, the Astronomer Royal's assumption as to intensity of light from two candles (supposed perfectly alike) is obviously legitimate ; at the same time I conceive (Mr. Moon's view) that the "amplitude of the vibration" (*not* its square) truly represents the other factor of the intensity formula.

This point can be easily tested experimentally as regards *sound*. Thus a tense string with amplitude of vibration $=1$ ought to become inaudible at twice the distance at which it ceases to be heard with amplitude $=0.70715$, if the simple power of the amplitude (not its *square*) be the correct assumption.

As regards the mathematical formula for the displacement of a particle of the æther in a wave, viz. $y = a \sin \frac{2\pi}{\lambda}(vt - x)$, I believe a represents the distance of the disturbed particle from its place of rest (not *necessarily*, therefore, the "*maximum vibration*," as stated by the Astronomer Royal, 'Undulatory Theory,' p. 7), and consequently that for an undulation comprising several waves of the same period but varying amplitudes, the formula should be

$$(a + a' + a'' \&c.) \sin \frac{2\pi}{\lambda}(vt - x), \text{ not } a^2 \sin \frac{2\pi}{\lambda}(vt - x).$$

Of course when there are waves of *different periods*, the expressions for each undulation (or series of such waves) must be kept distinct, and be added together to obtain the intensity.

HENRY HUDSON, M.D., M.R.I.A.

XX. *On the Optics of Mirage.* By Professor EVERETT, *M.A.,*
D.C.L., Queen's College, Belfast[*].

I PROPOSE in the present paper to investigate some of the
principles which govern the formation of images in a medium
of continuously varying index of refraction, with special reference
to the phenomena of mirage and atmospheric refraction.

I. I shall first establish the following proposition :—

In a medium in which the absolute index of refraction varies
continuously, the path of a ray will in general be curved; and its
curvature $\frac{1}{\rho}$ at any point is given by the formula

$$\frac{1}{\rho} = \frac{1}{\mu}\frac{d\mu}{dN} = \frac{d\log\mu}{dN},$$

N denoting distance measured along the normal towards the centre
of curvature, and therefore $\frac{d}{dN}$ denoting rate of increase in the
direction of the centre of curvature.

The simplest proof of this important proposition is derived
from the principle that μ varies inversely as v the velocity of
light.

Draw normal planes to a ray at two consecutive points of its
path. Then the distance of their intersection from either point
will be ρ, the radius of curvature. But these normal planes are
tangential to the wave-front in its two consecutive positions.

Hence it is easily shown by similar triangles that a very short line $d\mathrm{N}$ drawn from either of the points towards the centre of curvature, is to the whole length ρ of which it forms part, as dv the difference of the velocities of light at its two ends is to v the velocity at either end.   That is,

$$\frac{d\mathrm{N}}{\rho} = -\frac{dv}{v},$$

the negative sign being used because the velocity diminishes in approaching the centre of curvature.   But, since v varies inversely as μ, we have

$$-\frac{dv}{v} = \frac{d\mu}{\mu}.$$

Hence the curvature $\dfrac{1}{\rho}$ is given by any one of the four following expressions :—

$$\frac{1}{\rho} = -\frac{1}{v}\frac{dv}{d\mathrm{N}} = -\frac{d\log v}{d\mathrm{N}} = \frac{1}{\mu}\frac{d\mu}{d\mathrm{N}} = \frac{d\log\mu}{d\mathrm{N}}. \quad . \quad . \quad (\mathrm{A})$$

This proof is due to Professor James Thomson, who gave it in a paper to Section A at the recent Meeting of the British Association at Brighton.

It is obvious that the osculating plane is the plane which contains the direction of most rapid increase of index at the point considered, and that it cuts at right angles the surface-of-equal-index drawn through the point.

Cor. 1. The curvatures of different rays at the same point are directly as the rates of increase of μ in travelling along their respective normals.   If θ denote the angle which any ray makes with the surface-of-equal-index at the point, or which the radius of curvature of a ray makes with the direction of most rapid increase of index, the curvatures will be directly as the values of $\cos\theta$.   In fact, if $\dfrac{d\mu}{dr}$ denote the rate at which μ increases in a direction normal to the surfaces-of-equal-index, we have

$$\frac{d\mu}{d\mathrm{N}} = \frac{d\mu}{dr}\cos\theta,$$

and therefore

$$\frac{1}{\rho} = \frac{1}{\mu}\frac{d\mu}{dr}\cos\theta = \frac{d\log\mu}{dr}\cos\theta. \quad . \quad . \quad (\mathrm{B})$$

Cor. 2. When θ is small, and is regarded as a small quantity of the first order, $\cos\theta$, being approximately $1 - \frac{1}{2}\theta^2$, differs from unity by a small quantity of the second order.   Hence $\cos\theta$ is sensibly constant when θ is small, and rays which are but slightly

inclined to the surfaces-of-equal-index have sensibly the same curvature at the same point.

II. I now proceed to the solution of the following problem:—

Suppose a medium in which the index μ is a function of distance from a certain plane of reference which is itself a plane of maximum index. It is clear from (I.) that rays cutting this plane at any angle except a right angle will be bent back towards it, and may, under proper conditions, meet it again. Required the condition that rays cutting it at any small angle θ shall meet it again at a constant distance—that is to say, at a distance sensibly independent of θ. This is obviously the condition that rays of small inclination diverging from a point in the plane of reference, and lying in one and the same perpendicular plane, shall converge to a focus in the plane of reference.

Take rectangular axes of x and y in the common perpendicular plane, the axis of x being in and the axis of y perpendicular to the plane of reference. Let s denote distance measured along a ray; then $\frac{dx}{ds}$ or $\cos \theta$ is sensibly equal to unity. Also to the same degree of approximation we have

$$\theta = \tan \theta = \frac{dy}{dx},$$

$$\frac{1}{\rho} = -\frac{d\theta}{ds} = -\frac{d\theta}{dx} = -\frac{d^2y}{dx^2}. \quad \cdot \quad \cdot \quad \cdot \quad (C)$$

The problem which we have to solve has therefore been reduced to the following:—find what function $\frac{d^2y}{dx^2}$ must be of y that the increment of x from $y=0$ to the next occurrence of $y=0$ shall be independent of the maximum value of y. Mathematically considered, this is precisely the problem of finding what law of acceleration for a particle executing vibrations about a position of equilibrium will render the vibrations isochronous, y denoting the distance of the particle at time x from the position of equilibrium. Its solution is well known to be

$$\frac{d^2y}{dx^2} = -\frac{y}{a^2}, \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (D)$$

a being a constant; and the required law of index is therefore

$$\frac{d \log \mu}{dy} = -\frac{y}{a^2}. \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (E)$$

The general equation of a ray in the plane of x, y will be the
M 2

general integral of (D), namely

$$y = b \sin \frac{x-c}{a}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (F)$$

representing a curve of sines cutting the axis of x at points whose distances from the origin are

$$c, \quad c + \pi a, \quad c + 2\pi a, \ldots$$

The values of b and c vary from one ray to another; but a is the same for all; and hence the distance πa between two consecutive intersections of a ray with the axis of x is independent of the amplitude b. This constant quantity πa is evidently the focal length; and rays of small inclination diverging in the plane x, y from a point in the axis of x will converge to a series of foci at this constant distance apart. The same reasoning which proves that *all* small vibrations are isochronous, proves that wherever a plane of maximum index occurs, the other surfaces of equal index in its neighbourhood being also parallel planes, rays of small inclination diverging from a point in this plane must have a constant focal length; and it can be shown that the smallness of the aberration of rays from this geometrical focal length is especially promoted by symmetry of the medium about the plane of maximum index.

In fact, if we suppose $\log \mu$ and its differential coefficients with respect to y to be continuous, the assumption that the surfaces of equal index are parallel planes gives

$$\log \mu = A + By + Cy^2 + Dy^3 + \ldots,$$

whence

$$\frac{d \log \mu}{dy} = B + 2Cy + 3Dy^2 + \ldots$$

The assumption that y is measured from a plane of maximum index gives

$$B = 0, \quad 2C \text{ negative} = -\frac{1}{a^2} \text{ suppose.}$$

Hence, when y^2 is negligible, we have approximately

$$\frac{d \log \mu}{dy} = -\frac{y}{a^2},$$

which is identical with equation (E). The further supposition that the medium is symmetrical about the plane from which y is measured makes $D = 0$, because no odd powers of y can enter the expression for $\log \mu$.

III. The investigation in (II.) related to rays emanating from

a point in the plane of maximum index; but the law of index there deduced involves similar consequences for rays emanating from any other point; for the substitution of $x + \pi a$ for x in equation (F) merely changes the sign of y. Hence rays diverging from a point (x, y) will converge to a focus at the point $(x + \pi a, -y)$, then to another focus at the point $(x + 2\pi a, y)$, and so on.

It thus appears that in a medium in which the law (E) prevails, every object will yield a series of real images, alternately inverted and erect, πa being their common distance asunder in a direction parallel to the plane of maximum index; while, as regards distances measured normal to the plane of maximum index, each image in the series corresponds to the reflected image of its predecessor with respect to this plane. It is of course to be understood that the images are formed in one dimension only, like those formed by a cylindrical lens.

I may remark incidentally that in a medium in which the surfaces-of-equal-index are parallel planes, if one ray of small inclination to these planes is a curve of sines, all rays of the same or less amplitude must also be curves of sines; for equation (D) cannot hold for one ray unless (E) holds for all distances from the plane of reference not exceeding the amplitude of that ray.

I may also remark that a prism-like or lens-like arrangement of surfaces-of-equal-index produces less deviation in rays than an arrangement in which these surfaces are approximately parallel to the course of the rays. This is obvious from equation (B), which shows that, for a constant rate of variation of $\log \mu$ normally to the surfaces-of-equal-index, the curvature is proportional to $\cos \theta$.

The fact that rays emanating from *any* point in the medium converge to a focus, and that the focal length measured parallel to the axis of x has a constant value, corresponds to the self-evident proposition in cycloidal oscillation, that the time from *any* point to the symmetrically situated point on the other side, when one of the extreme positions is taken in the interval, is the half-period of oscillation.

If, instead of supposing the surfaces-of-equal-index to be parallel planes, we suppose μ to be a function both of y and x, then $\dfrac{d^2 y}{dx^2}$ will be a function both of y and x, and the analogous supposition in cycloidal oscillation is that of gravity varying with time. On this supposition, it is clear that if two particles start at the same instant with different velocities from the *lowest points* of two equal cycloids, they will keep time with each other, however great and sudden the variations of gravity may be sup-

posed to be*, unless these variations be such as to compel one of the particles to travel beyond the cusp of its cycloid and thus introduce discontinuity.

As velocity in the case of the particle corresponds to $\frac{dy}{dx}$ in the case of a ray, the corresponding inference is that, in the medium now supposed, rays which proceed from the same point in the axis of x at different inclinations to this axis will meet it again at the same distance. The conclusion cannot be extended to points above or below the axis of x.

IV. When the surfaces-of-equal-index are parallel planes, the deviation of a ray in passing from one of these surfaces to another can be expressed in terms of the angle of incidence at the first surface and the relative index from the first surface to the last—being entirely independent of the distance between the two surfaces, and of the character of the intervening layers.

For, since the axis of y is perpendicular to the planes-of-equal-index, equation (B) becomes

$$\frac{1}{\rho} = - \frac{d \log \mu}{dy} \cos \theta.$$

Hence

$$d\theta = - \frac{ds}{\rho} = \frac{ds}{dy} \cos \theta \, d \log \mu = \frac{\cos \theta}{\sin \theta} \, d \log \mu, \quad . \quad (G)$$

or

$$d \log \mu = \frac{\sin \theta \, d\theta}{\cos \theta} = - d \log \cos \theta.$$

Integrating from μ_1, θ_1 at the first surface to μ_2, θ_2 at the last, we have

$$\frac{\mu_2}{\mu_1} = \frac{\cos \theta_1}{\cos \theta_2}. \quad . \quad . \quad . \quad . \quad . \quad (H)$$

When the change of index is abrupt, this equation amounts to a statement of the " law of sines;" for $\cos \theta_1$ is the sine of the angle of incidence, $\cos \theta_2$ is the sine of the angle of refraction, and $\frac{\mu_2}{\mu_1}$ is the relative index from the first medium into the second. Instead of integrating between limits, we might have deduced the general integral

$$\mu \cos \theta = \text{constant},$$

which applies to the whole course of the ray.

---

* For such changes will not disturb the equalities, Ratio of accelerations = Ratio of velocities = Ratio of distances from vertex = Constant.

V. Putting $\theta_2 = 0$ in equation (H), we have

$$\frac{\mu_2}{\mu_1} = \cos \theta_1.$$

Hence a ray entering at any angle θ_1 to the parallel planes-of-equal-index will become parallel to these planes when it has penetrated as far as the plane in which

$$u = \mu_1 \cos \theta_1. \quad . \quad . \quad . \quad . \quad . \quad \text{(K)}$$

It will then be bent back symmetrically, and will emerge again from the plane at which it entered, making the angle of emergence equal to the angle of incidence. This result can only occur when the original course of the ray is from greater index to less.

When there are two regions of constant indices μ_1, μ_2 (μ_1 being the greater) separated by a region in which μ diminishes continuously from μ_1 to μ_2 (the surfaces-of-equal-index being parallel planes), a ray entering this intermediate region from the side where μ is greatest will be able to get through if $\cos \theta_1$ is less than $\frac{\mu_2}{\mu_1}$. But if $\cos \theta_1$ is greater than $\frac{\mu_2}{\mu_1}$, there will be a plane in the intermediate region in which equation (K) will be satisfied, and the ray will be returned from this plane. When the change of index is abrupt, the above statement resolves itself into the usual formula for the " critical angle " of total reflection.

VI. Thus far we have been supposing the surfaces-of-equal-index to be plane. If we now suppose them to be horizontal surfaces parallel to the general surface of the earth, it will be necessary to modify the conditions of (II.) by making the axis of x not a straight but a horizontal line, which, if we regard the earth as a sphere, will be a circular arc described about the earth's centre; while the ordinates denoted by y will not be parallel to any one line, but will be vertical, and will therefore be everywhere perpendicular to the axis of x.

Putting R for the earth's radius, equations (C) will now stand thus :—

$$\theta = \tan \theta = \frac{dy}{dx},$$

$$\frac{1}{\rho} = \frac{1}{R} - \frac{d\theta}{ds} = \frac{1}{R} - \frac{d\theta}{dx} = \frac{1}{R} - \frac{d^2y}{dx^2};$$

whence, putting for $\frac{1}{\rho}$ its value $-\frac{d \log \mu}{dy}$, we have

$$\frac{d^2y}{dx^2} = \frac{1}{R} + \frac{d \log \mu}{dy}.$$

To obtain the convergence which the problem requires, we must have

$$\frac{d^2y}{dx^2} = -\frac{y}{a^2}; \quad \cdots \quad \text{(D)}$$

for the general equation of the curved path of a ray will then have the same form as before, namely

$$y = b \sin \frac{x-c}{a}; \quad \cdots \quad \text{(F)}$$

and it is upon this form that the convergence which the problem requires alone depends. For the same reason, the consequences deduced in (III.) will remain completely applicable.

Equation (D) now denotes the curve obtained by making equal algebraic additions to the curvatures at all points of a curve of sines, or by bending uniformly a rectangular rod in the plane of one of its faces which has a curve of sines drawn upon it.

The required law of variation of index is

$$\frac{d \log \mu}{dy} = -\frac{y}{a^2} - \frac{1}{R}.$$

At the point where a ray cuts the axis of x we have

$$\frac{d \log \mu}{dy} = -\frac{1}{R}.$$

The axis of x, therefore, does not lie in the surface of maximum index, but in that surface-of-equal-index which possesses the property that rays cutting it at a small inclination have, at the points of section, a curvature equal to that of the earth. In consequence of this property, rays can travel for any distance along that surface-of-equal-index which contains the axis of x; whereas rays above it have greater curvature and bend down to meet it, while on the other hand rays below it are less curved (or may even be curved in the opposite direction), and it accordingly bends down to meet them.

To find the level of no curvature, or of maximum index, we must put $\dfrac{d \log \mu}{dy} = 0$, an equation which gives

$$y = -\frac{a^2}{R}.$$

The depth of the surface of maximum index below the axis of x is therefore a third proportional to the earth's radius and the parameter a. Rays above this level are curved in the same direction as the earth; those below it are curved in the opposite direction. The value of $\dfrac{a^2}{R}$ is about five feet when the value of

a is about two miles, or when the focal length πa is about six miles.

VII. We now proceed to the consideration of the physical circumstances on which the variation of index in the atmosphere depends.

The experiments of Biot and Arago have shown that, for variations of density due either to change of temperature or change of pressure, $\mu-1$ varies directly as the density; and it further appears from the experiments of Jamin, that at ordinary temperatures the value of $\mu-1$ is sensibly the same for dry as for saturated air at the same density. If α denote the coefficient of expansion $\cdot00366$ or $\frac{1}{273}$, and h the pressure expressed in millimetres of mercury, the formula for $\mu-1$ is

$$\mu-1=\frac{\cdot0002943}{1+\alpha t}\cdot\frac{h}{760};$$

and this may also be regarded as the value of $\log\mu$, since the difference $\frac{1}{3}(\mu-1)^3-$ &c. is too small to be appreciable. Hence, for horizontal or nearly horizontal rays, the curvature $\frac{1}{\rho}$ or $-\dfrac{d\log\mu}{dy}$ at any point is

$$\frac{1}{\rho}=\frac{\cdot0002943}{760}\left\{-\frac{dh}{dy}\frac{1}{1+\alpha t}+\frac{dt}{dy}\frac{h\alpha}{(1+\alpha t)^2}\right\}.$$

But $-\dfrac{dh}{dy}$, being the fall of the barometric column per unit of ascent, is equal to $\dfrac{h}{H}$, where H denotes the height of the homogeneous atmosphere, which height, if we neglect variations of gravity, is $26200\,(1+\alpha t)$ feet. We have therefore, if we make the foot the unit of measurement for y and ρ,

$$\frac{1}{\rho}=\cdot0002943\cdot\frac{h}{760}\cdot\frac{1}{(1+\alpha t)^2}\left\{\frac{1}{26200}+\frac{1}{273}\frac{dt}{dy}\right\}$$

$$=\frac{1}{89000000}\cdot\frac{h}{760}\cdot\frac{1}{(1+\alpha t)^2}\left\{1+96\frac{dt}{dy}\right\}.$$

This expression vanishes when $\dfrac{dt}{dy}=-\dfrac{1}{96}$. Hence, when the diminution of temperature per foot of ascent is $\frac{1}{96}$ of a degree Centigrade, the density of the air is uniform and rays are straight. When the decrease of temperature upwards is more rapid than this, the upper air is the denser, and rays are bent upwards, in other words, their curvature is opposite to that of the earth.

The rate of decrease usually assumed as an average is $\frac{1}{300}$ of a degree Fahrenheit, or $\frac{1}{540}$ of a degree Cent. per foot. This will give, at $0°$ C. and 760 millims.,

$$\frac{1}{\rho} = \frac{1}{89000000} \left\{ 1 - \frac{96}{540} \right\},$$

whence $\rho = 108000000$ feet, or $5 \cdot 2$ radii of the earth.

At $10°$ C., 760 millims., and the same rate of decrease as above, ρ is equal to about $5 \cdot 6$ radii of the earth, and the correction for refraction in levelling is therefore $\frac{1}{5 \cdot 6}$ of the correction for the curvature of the earth. Rankine (Rules and Tables, p. 131) says, "The correction for refraction to be added to the reading is very variable and uncertain. On an average, it may be taken at one sixth of the correction for curvature."

In order that the curvature of a ray may be the same as that of the earth at $0°$ C. and 760 millims., the expression $1 + 96 \frac{dt}{dy}$ must be equal to 89000000 feet divided by the earth's radius— that is, to about $4 \cdot 26$. We shall therefore have

$$\frac{dt}{dy} = \frac{3 \cdot 26}{96} = \frac{1}{29 \cdot 4};$$

that is, the temperature must *increase* at the rate of $1°$ C. for $29 \cdot 4$ feet of ascent, or $1°$ F. for $16 \cdot 3$ feet. Any portion of the earth with such a state of things prevailing over it will appear plane, distant objects being no longer hidden by the intervening convexity. A still more rapid increase will make the surface of the earth appear concave.

VIII. I shall now take up in detail the principal phenomena of mirage, and indicate what I conceive to be their correct explanations.

1. An unusual extension of the range of vision, like that described by Latham in the Philosophical Transactions for 1798, when the coast of France from Calais to the neighbourhood of Dieppe was clearly visible from Hastings.

Explanation.—An increase of temperature with ascent, producing an exaggeration of the ordinary downward curvature of rays, as explained in the preceding section.

2. Distant objects seen inverted above their true positions. Some instances of this form of mirage are described by Vince in the "Bakerian Lecture," Phil. Trans. 1799; many more are described by Scoresby from his observations in the Arctic regions; and the phenomenon is extremely common across extensive sheets of calm water. Usually two images are seen, namely an erect image in the true or what appears to be the true position, and an inverted image above it. Sometimes, however, the

inverted image is visible when the erect image is hidden by the convexity of the intervening water.

Explanation.—A very rapid increase of temperature upwards in a stratum of air overhead. The inverted images are formed by rays incident from below upon this stratum at such an obliquity that (as explained in V.) they cannot get through, but are compelled to descend again, and thus undergo a kind of reflection. The upward increase is supposed to be more sudden here than in (1), and confined to a thinner stratum.

3. Multiple images seen above the true position of the object.

Explanation.—Either several strata of rapid upward increase of temperature, each of them, as in (2), fulfilling the office of a mirror, or a single such stratum of irregular shape, yielding reflections in different places.

4. An appearance as of architectural columns, obelisks, spires, or basaltic cliffs. Such appearances are said to be common in the illusions of the *Fata Morgana* at the Straits of Messina; and many instances are described, with illustrative plates, in Scoresby's 'Greenland.' Such appearances are always due to the vertical magnification of real objects.

Explanation.—In the arrangements of III. and VI. an object at a short distance in front of or behind a focus conjugate to the position of the observer's eye, will be greatly magnified in the vertical direction, its vertical diameter being seen under the same angle as if the eye were at this conjugate focus. If between the eye and the first conjugate focus, it will appear erect; if between the first and the second conjugate focus, inverted. I believe that, when vertical magnification is exhibited with any thing like regularity, an arrangement approximately resembling that described in (VI.) prevails in the body of air which lies between the observer and the objects magnified.

The same appearance is often seen on land (small bushes, for example, being magnified into tall trees), and is to be similarly explained.

5. A false appearance of water in a place actually occupied by hot and dry ground.

Explanation.—An increase of temperature downwards, within a few inches of the ground, at a rate considerably exceeding $\frac{1}{96}$ of a degree Cent. per foot. This will produce an upward bending of rays and a *quasi* reflection, as in (2); but the seeming mirror is now below the observer's eye instead of above it. The flickering movements of the reflected images thus seen, due to currents of hotter and colder air, greatly resemble the appearances produced by the rippling of waves on a lake; but probably the most irresistible feature in the illusion is the gleam of

the reflected sky. The sky itself, and its reflection in water, so far exceed in brightness all other objects in an ordinary landscape, that when this gleam is seen in a place where the sky cannot be, the observer feels irresistibly compelled to ascribe it to water.

IX. In the transmission of rays through a medium of continuously varying index, no proper distinction can be taken between refraction and reflection. They shade insensibly into one another; or rather, I should perhaps say, both names are equally inappropriate in this application.

X. The following are some of the mistakes which have frequently been made by writers on mirage :—

1. The mistake of supposing that a ray in air can be bent at an angle—in other words, can have a point of infinite curvature. This would imply an absolutely abrupt change of index.

2. The mistake of supposing that a ray can pursue a straight course parallel to planes of equal index in a continuously varying medium. The contrary was pointed out so long ago as 1799 and 1800 by Vince and Wollaston in the Philosophical Transactions, but appears to have since dropped out of mind.

3. The mistake of supposing that rays which first ascend and then descend, or which first descend and then ascend, must produce inverted images, or an appearance as of reflection. If all the rays of a system are circular arcs in vertical planes, with the same radius of curvature, and everywhere nearly horizontal, the images which they present to the eye will be neither inverted nor distorted, but simply elevated or depressed; for such a system can be converted into a system of straight rays by a process of bending which will not alter their distances apart. Let such of them as lie in one vertical plane be represented by a diagram drawn upon one face of a prismatic rod; then if the rod be bent in the plane of this face with a uniform curvature opposite to that of the lines of the diagram, all these lines will become straight; and it is clear that this process does not sensibly alter the distances between the lines, nor the angles at which they intersect each other.

Some further consequences of the law of ray-curvature, of theoretical rather than practical interest, are reserved for a second paper.

XXI. *Correction to a Paper " On an Experimental Determina-
tion of the Relation between the Energy and Apparent Intensity
of Sounds of different Pitch."* By R. H. M. BOSANQUET,
Fellow of St. John's College, Oxford*.

IN the paper above referred to it was sought to establish
directly, by experiment, the following relation :—" The ap-
parent intensity of a musical note is proportional to the mecha-
nical energy expended in the production of the tone, and in-
versely as the wave-length or periodic time." At the end of
the paper certain deductions were made from this law, with
reference to the relations connecting amplitude, pitch, and pe-
riodic time with apparent intensity. The writer has to acknow-
ledge with regret that the reasoning of this latter portion is
erroneous. The error does not seem to be very obvious, as no
one has yet pointed it out, to the writer's knowledge. It was
intended to wait until some measures could be completed by an
improved method, and to make a more complete communication.
But as the performance of this has been unavoidably delayed,
and the writer finds the erroneous result quoted as correct by
Dr. Mayer, in his interesting paper, Phil. Mag. Feb. 1873, it is
desirable that the mistake should be corrected at once.

The error committed was precisely of the nature against which
a warning was given on the last page of the paper. It was
assumed that the energy of the vibration of the plate of air con-
sidered was identical with the total energy transferred through
the plate. The assumption was simply a *non sequitur*. The
following is believed to be correct.

To find the total energy of the sound which crosses a given
plane section in one second, in terms of the amplitude and wave-
length.

Let the sound consist of a succession of plane waves generated
at one end of a straight cylindrical tube having the given section.
The waves then traverse the cylinder without sensible diminution
of intensity, if we neglect the influence of the walls. Let the
delivery of the sound commence at the beginning of a second
and continue throughout the second, and suppose that the con-
dition of the air in the tube at the expiration of the second is
photographed. Then the sum of the potential and kinetic
energy of the disturbed air in the tube, through a length equal
to the velocity of sound per second, is the energy supplied from
the source in one second.

First for the potential energy. This consists partly of com-
pressions and partly of dilatations, both of which are supplied

from the source: the sum of their absolute numerical values is therefore to be taken.

Consider a disk of air of section unity and thickness dx; let this be compressed, the section remaining the same, and its thickness becoming $dx - dy$. Then the increment of pressure is $\frac{dy}{dx} \times$ atmospheric pressure $\times$ thermometric and specific-heat corrections. The atmospheric pressure $\times$ corrections for heat $= v^2\rho$; where v^2 is the coefficient of the equation of the transmission of sound, and ρ the mean density of the atmosphere. Hence the pressure exerted by the compressed disk over and above the atmospheric pressure is $\frac{dy}{dx} . v^2 . \rho$.

As the compression proceeds through the small distance δdy, an element of work is done

$$= \frac{dy}{dx} . v^2 . \rho . \delta dy;$$

and the sum of all the work done in the compression from $dy = 0$ up to $dy = dy$ is

$$\int_0^{dy} \frac{dy}{dx} . v^2 . \rho . \delta dy = \frac{1}{2} \frac{dy^2}{dx} . v^2 \rho.$$

If we now assume that the sound in the tube consists of a simple periodic vibration

$$y = a \sin \frac{2\pi}{\lambda} (vt - x),$$

we have for the work stored in any disk dx, at a distance x from the end of the tube,

$$\frac{v^2\rho}{2} \frac{dy^2}{dx^2} . dx = \frac{v^2\rho}{2} \left(\frac{2\pi a}{\lambda}\right)^2 \cos^2 \frac{2\pi}{\lambda} (vt - x) dx.$$

And the work stored in all the disks in a quarter wave-length, or, if P be average potential work in unit of length,

$$\text{P} . \frac{\lambda}{4} = \frac{v^2\rho}{2} \left(\frac{2\pi a}{\lambda}\right)^2 \int_0^{\frac{\lambda}{4}} \cos^2 \frac{2\pi}{\lambda} (vt - x) dx,$$

$$\text{,,} \qquad \text{,,} \qquad \int_0^{\frac{\lambda}{4}} \frac{1}{2} \left(1 + \cos 2 . \frac{2\pi}{\lambda} (vt - x)\right) dx,$$

$$\text{,,} \qquad \text{,,} \qquad \frac{1}{2} \left\{ \frac{\lambda}{4} - \frac{\lambda}{4\pi} \left[\sin 2 . \frac{2\pi}{\lambda} (vt - x) \right]_0^{\frac{\lambda}{4}} \right\}.$$

And if we take $vt - x = 0$ at the lower limit,

$$P = \left(\frac{\pi \cdot a \cdot v}{\lambda} \right)^2 \rho.$$

And the potential work in the length v traversed by the sound in one second $= v^3 \rho \left(\frac{\pi a}{\lambda} \right)^2$.

This is expressed in foot-pounds; for $v^2 \rho$ is in terms of the atmospheric pressure over the unit section, v is a number of feet, and $\frac{a}{\lambda}$ a number.

We have now to estimate the kinetic energy of each disk of the photographed column.

Kinetic energy of disk dx

$$= \frac{1}{2} \left(\frac{dy}{dt} \right)^2 \rho \, dx$$

$$= \frac{1}{2} v^2 \rho \left(\frac{2\pi a}{\lambda} \right)^2 \cos^2 \frac{2\pi}{\lambda} (vt - x) dx,$$

which is identically the same expression we had before. We have therefore simply to double the above result; and we get for the total energy delivered in one second,

$$2 \rho v^3 \left(\frac{\pi a}{\lambda} \right)^2.$$

If we apply to this result the experimental law, that the apparent intensity is directly as the energy and inversely as the wavelength or periodic time, we have

$$I = \text{const.} \frac{a^2}{\lambda^3}.$$

And instead of the laws given in the writer's communication of last November, we have the following :—

The apparent intensities of sounds of different pitch are proportional to the squares of the amplitudes, and inversely as the cubes of the wave-lengths.

In sounds of the same apparent intensity, the squares of the amplitudes vary as the cubes of the wave-lengths.

XXII. *On the Effect of Internal Friction on Resonance.*
By J. Hopkinson, D.Sc., B.A.[*]

A S a typical case which may be taken as illustrating the
nature of the phenomena in more complex cases, let us
consider the motion of a string, of a column of air, or an elastic
rod vibrating longitudinally, one extremity being fixed, whilst
the other is acted on so that its motion is expressed by a simple
harmonic function of the time.

Let l be the length of the string, a the velocity with which a
wave is transmitted along it, ξ the displacement of a point of the
string distant x from the fixed extremity at the time t. In the
hypothetical case, in which there is no friction, no resistance of
a surrounding medium, and the displacements are indefinitely
small, the equation of motion is

$$\frac{d^2\xi}{dt^2} = a^2 \frac{d^2\xi}{dx^2}, \quad \cdots \quad \cdots \quad (1)$$

with the conditions that at the extremities $\xi = 0$ when $x = 0$,
and $\xi = A \sin nt$ when $x = l$, also that at some epoch ξ shall be
a specified function of x.

If we start with the string straight and at rest, we have the
condition $\xi = 0$ for all values of x from zero to very near l when
$t = 0$, and we readily find

$$\xi = \frac{A}{\sin \frac{nl}{a}} \sin nt \cdot \sin \frac{nx}{a} + \Sigma C_p \sin \frac{p\pi x}{l} \cdot \sin \frac{p\pi a t}{l}, \quad \cdots \quad (2)$$

where $C_p = (-1)^p \dfrac{2nal}{n^2 l^2 - p^2 \pi^2 a^2}$.

When $\frac{nl}{a}$ is very nearly a multiple of π (*i. e.* when the note
sounded by the forcing vibration at the extremity is almost the
same as one of the natural notes of the string), we have two notes
sounded with intensity, viz. one the same as the forcing vibra-
tion, the other native to the string. That this is the case may
be readily seen with a two-stringed monochord, the strings being
nearly in unison : one string being sounded, the motion of the
other is seen by the eye to be intermittent, the period of varia-
tion being the same as that of the beats of the two strings
sounded together. But should $\frac{nl}{a}$ be an exact multiple of π,
two terms in the value of ξ become infinite, and our whole me-
thod of solution is invalid. A somewhat similar difficulty, of

course, occurs in the lunar and planetary theories, but with this difference : there the difficulty is introduced by the method of solving the differential equation, and is avoided by modifying the first approximation to a solution ; here it is inherent in the differential equation, and can only be avoided by making that equation express more completely the physical circumstances of the motion.   One or more of the assumptions on which the differential equation rests is invalid.   We must look either to terms of higher orders of smallness, to resistance of the air, or to internal friction. With the modifications due to the last cause we are now concerned.

The approximate effect of internal friction is probably to add to the stress $E \dfrac{d\xi}{dx}$, produced by the strain $\dfrac{d\xi}{dx}$ when the parts of the body are relatively at rest, a term proportional to the rate at which the strain is changing ; so that the stress when there is relative motion will be $E \left(\dfrac{d\xi}{dx} + k \dfrac{d^2\xi}{dx\, dt} \right)$, and our equation of motion becomes

$$\frac{d^2\xi}{dt^2} = a^2 \left(\frac{d^2\xi}{dx^2} + k \frac{d^3\xi}{dx^2\, dt^2} \right). \quad . \quad . \quad . \quad . \quad (3)$$

The solution of this equation will contain two classes of terms. First, a series corresponding to those under the sign of summation in (2), which principally differ from (2) in the coefficients decreasing in geometrical progression with the time, the highest fastest, and in the total absence of the notes above a certain order as periodic terms ; these terms we may consider as wholly resulting from the initial conditions, and as having no permanent effect on the motion. Second, a term corresponding to the first term of (2), and which expresses the state of steady vibration when work enough is continually done by the forced vibration of the extremity to maintain a constant amplitude.   The investigation of this term is a little more troublesome, because the motion is periodic, the effect of friction being to alter the motion in a manner dependent on the position of the point, not on the time, and equation (3) cannot be satisfied by a sine or a cosine alone of the time.

Assume $\xi = \phi(x) \sin mt + \psi(x) \cos mt,$

or a series of such terms, if possible, each pair satisfying equation (3). Substitute in the equations of motion, and equate coefficients of $\sin mt$ and $\cos mt$,

$$\left. \begin{array}{l} a^2(\phi'' - km\psi'') = -m^2\phi, \\ a^2(\psi'' + km\phi'') = -m^2\psi. \end{array} \right\} \quad . \quad . \quad . \quad (4)$$

Assume

$$\left.\begin{array}{l}\phi = c_1 \sin \lambda x, \\ \psi = c_2 \sin \lambda x, \end{array}\right\} \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (5)$$

where c_1, c_2, and λ may be imaginary, but ϕ and ψ are real: this form is indicated as suitable, because ξ must change sign with x.

We obtain

$$\left.\begin{array}{l}a^2(c_1 - c_2 km)\lambda^2 = m^2 c_1, \\ a^2(c_2 + c_1 km)\lambda^2 = m^2 c_2 \,; \end{array}\right\} \quad \cdot \quad \cdot \quad \cdot \quad (6)$$

whence

$$c_1^2 = -c_2^2, \quad c_2 = \pm c_1 \sqrt{-1}\,; \quad \cdot \quad \cdot \quad (7)$$

and

$$\lambda = \pm \mu \left(1 \pm \sqrt{-1}\, \tan \frac{\theta}{2}\right),$$

where $\tan \theta = km$,

$$\mu = \frac{m}{a}\, \frac{\cos \dfrac{\theta}{2}}{\sqrt[4]{1 + k^2 m^2}}.$$

The most general real expression for ϕ is then

$$\frac{A_1 + B_1 \sqrt{-1}}{2} \sin \mu \left(1 + \sqrt{-1}\, \tan \frac{\theta}{2}\right) x$$

$$+ \frac{A_1 - B_1 \sqrt{-1}}{2} \sin \mu \left(1 - \sqrt{-1}\, \tan \frac{\theta}{2}\right) x\,;$$

or, as it may be written,

$$\left.\begin{array}{l}\phi = A_1 \sin \mu x \cdot \dfrac{\epsilon^{\mu \tan \frac{\theta}{2} \cdot x} + \epsilon^{-\mu \tan \frac{\theta}{2} \cdot x}}{2} \\[3mm] \qquad + B_1 \cos \mu x \cdot \dfrac{\epsilon^{\mu \tan \frac{\theta}{2} \cdot x} - \epsilon^{-\mu \tan \frac{\theta}{2} \cdot x}}{2} \, . \end{array}\right\} \quad \cdot \quad \cdot \quad (8)$$

Similarly

$$\psi = A_2 \sin \mu x \cdot \frac{\epsilon^{\mu \tan \frac{\theta}{2} \cdot x} + \epsilon^{-\mu \tan \frac{\theta}{2} \cdot x}}{2}$$

$$+ B_2 \cos \mu x \cdot \frac{\epsilon^{\mu \tan \frac{\theta}{2} \cdot x} - \epsilon^{-\mu \tan \frac{\theta}{2} \cdot x}}{2} \, .$$

The constants will be connected by the relations

$$\left\{\begin{array}{l} A_1 + B_1 \sqrt{-1} = \quad A_2 \sqrt{-1} - B_2, \\ A_1 - B_1 \sqrt{-1} = -A_2 \sqrt{-1} - B_2\,; \end{array}\right.$$

that is,

$$A_1 = -B_2 \quad \text{and} \quad B_1 = A_2. \quad \cdot \quad \cdot \quad \cdot \quad (9)$$

Let

$$P = \sin \mu l \cdot \frac{\epsilon^{\mu \tan \frac{\theta}{2} \cdot l} + \epsilon^{-\mu \tan \frac{\theta}{2} \cdot l}}{2}, \left.\begin{array}{c} \\ \\ \\ \\ \end{array}\right\} \quad . \quad (10)$$

$$Q = \cos \mu l \cdot \frac{\epsilon^{\mu \tan \frac{\theta}{2} \cdot l} - \epsilon^{-\mu \tan \frac{\theta}{2} \cdot l}}{2}.$$

If possible, let m be other than n; when $x = l$, we have $\phi = 0$ and $\psi = 0$, or

$$\begin{cases} A_1 P + B_1 Q = 0, \\ B_1 P - A_1 Q = 0; \end{cases}$$

therefore, since A_1, B_1 must be real, they must vanish, and we conclude that the only steady vibration is of the same period as that impressed on the extremity.

Let $m = n$; when $x = l$, $\phi = A$ and $\psi = 0$; hence

$$\begin{array}{c} PA_1 + QB_1 = A, \\ PB_1 - QA_1 = 0; \end{array}\Bigg\}$$

$$A_1 = \frac{AP}{P^2 + Q^2}, \left.\begin{array}{c} \\ \\ \\ \end{array}\right\} \quad . \quad . \quad . \quad . \quad (11)$$

$$B_1 = \frac{AQ}{P^2 + Q^2}.$$

This completely determines the steady vibration of the string.

Suppose a change to take place in the forcing vibration, it is easy to see that the result will be that momentarily all the notes natural to the string with both ends fixed will be sounded. This conclusion could readily be tested by graphically describing the motion of a point of a string moving in the manner supposed, the motion being produced by a tuning-fork actuated by an electromagnet. If this be verified, an attempt might be made to determine the value of k for various strings or wires by comparing the amplitude of vibration at the points of greatest and least vibration; and at the different points of least vibration true nodes will not occur. The curve having x for abscissa, and the maximum value of ξ at each point for ordinate, might possibly be portrayed by photographing a vibrating string. The calculations would be much facilitated by the fact that $\mu = \dfrac{n}{a}$ if small quantities of the second order are neglected. Suppose that $\mu l = 2\pi$, a case of strong resonance; then $P = 0$ and $Q = \pi k n$ very nearly; we have $A_1 = 0$ and $B_1 = \dfrac{A}{\pi k n}$, and the motion is

expressed by the equation

$$\xi = \frac{A}{\pi k n}\left\{ \frac{kn^2 x}{a}\cos\frac{nx}{a}\sin nt + \sin\frac{nx}{a}\cos nt \right\}.$$

Let the amplitudes observed at the node and middle of ventral segments of the string be α, β; we have

$$\left.\begin{array}{l} \alpha = \dfrac{Anl}{2\pi a}, \\[2mm] \beta = \dfrac{A}{\pi k n}; \end{array}\right\} \qquad \cdots \quad \cdots \quad (12)$$

therefore

$$k = \frac{2\alpha}{\beta}\frac{a}{n^2 l} = \frac{\alpha}{\beta}\frac{1}{\pi n},$$

the result being expressed in seconds. It is worth noticing that the vibrations throughout the ventral segments in this case are nearly a quarter of vibration behind the extremity in phase. If the theory of friction here applied be correct, many important facts could follow from a determination of the value of k in different substances—for example, the relative duration of the harmonics of a piano-wire.

Let us now calculate what is the work done by the force maintaining the vibration of the extremity. The force there exerted is

$$E\left(\frac{d\xi}{dx} + k\frac{d^2\xi}{dx\,dt}\right),$$

and the work done in time dt is

$$E\left(\frac{d\xi}{dx} + k\frac{d^2\xi}{dx\,dt}\right)\frac{d\xi}{dt}\,dt,$$

x being put equal to l after differentiation. We have then work done from time 0 to time t

$$= \int_0^t \left\{ E\left(\frac{d\xi}{dx} + k\frac{d^2\xi}{dx\,dt}\right)\frac{d\xi}{dt} \right\}_{x=l} dt.$$

In estimating the work done in any considerable period, we may exclude the periodic terms as unimportant. Hence work done on extremity of string

$$= \frac{nEt}{2}\left\{ \left(\frac{d\psi}{dx} + kn\frac{d\phi}{dx}\right)\phi - \left(\frac{d\phi}{dx} - kn\frac{d\psi}{dx}\right)\psi \right\}_{x=l}$$

$$= \frac{nEt}{2}\left\{ \frac{d\psi}{dx} + kn\frac{d\phi}{dx} \right\}_{(x=l)} A.$$

An expression for this could of course be at once written down without approximation; but the case where k is small is most important; then we have

$$\left\{ \begin{array}{l} P = \sin \dfrac{nl}{a}, \\ Q = \cos \dfrac{nl}{a} \cdot \dfrac{n^2 lk}{2a}, \end{array} \right.$$

$$\left\{ \begin{array}{l} A_1 = \dfrac{A}{\sin \dfrac{nl}{a}}, \\ B_1 = \dfrac{A \cos \dfrac{nl}{a}}{\sin^2 \dfrac{nl}{a}} \cdot \dfrac{n^2 lk}{2a}, \end{array} \right.$$

unless $\sin \dfrac{nl}{a}$ becomes very small,

$$\left\{ \begin{array}{l} \phi = A \dfrac{\sin \dfrac{nx}{a}}{\sin \dfrac{nl}{a}}, \\ \psi = \dfrac{A}{\sin^2 \dfrac{nl}{a}} \cdot \dfrac{kn^2}{2a} \left\{ l \cos \dfrac{nl}{a} \cdot \sin \dfrac{nx}{a} \right. \\ \qquad\qquad \left. - x \cos \dfrac{nx}{a} \sin \dfrac{nl}{a} \right\}. \end{array} \right.$$

Work done on the string

$$= \dfrac{n^3 Etk}{4a \sin^2 \dfrac{nl}{a}} A^2 \left\{ l \cos^2 \dfrac{nl}{a} \cdot \dfrac{n}{a} - \cos \dfrac{nl}{a} \sin \dfrac{nl}{a} \right.$$

$$\left. + l \dfrac{n}{a} \sin^2 \dfrac{nl}{a} + 2 \sin \dfrac{nl}{a} \cos \dfrac{nl}{a} \right\}$$

$$= \dfrac{n^3 Etk A^2}{4a \sin^2 \dfrac{nl}{a}} \left\{ \dfrac{nl}{a} + \sin \dfrac{nl}{a} \cos \dfrac{nl}{a} \right\}.$$

If

$$\sin\frac{nl}{a}=0, \quad Q=\pm\frac{n^2lk}{2a},$$

$$A_1=0, \text{ and } B_1=\pm\frac{2a}{n^2lk}A,$$

$$\left\{\begin{array}{l} \phi=\pm\dfrac{A}{l}x\cos\dfrac{nx}{a}, \\[2mm] \psi=\pm\dfrac{2aA}{n^2lk}\sin\dfrac{nx}{a}. \end{array}\right.$$

Work done $=\dfrac{A^2}{lk}Et.$

We infer that the energy imparted to the string varies as the square of the amplitude of vibration of the extremity, that it rapidly increases as the period approaches that of the string, that, if these periods differ materially, the work is directly proportional to the friction and increases rapidly with the number of vibrations—but that if the periods are identical, the work varies *inversely* as the friction, the diminishing of the friction being more than counterbalanced by the increased amplitude.

It is interesting to examine how this energy is distributed over the string. This is easily done by writing down the work done by one portion of the string from x to l, on the remainder from 0 to a, and then taking the differential; we readily find that work absorbed by portion dx of string

$$=\frac{n^2kEt}{2}\left(\frac{d\phi}{dx}\Big|^2+\frac{d\psi}{dx}\Big|^2\right)dx.$$

Substituting, we obtain, when the string does not resonate,

$$\text{work}=\frac{n^4kEt}{2a}\frac{\cos^2\dfrac{nx}{a}}{\sin^2\dfrac{nl}{a}}A^2\,dx;$$

when the string resonates,

$$=\frac{Et}{l^2k}\cos^2\frac{nx}{a}\cdot A^2dx.$$

In either case the absorption of energy, and therefore the heating-effect, is greatest at the nodes, and, omitting squares of k, vanishes at the middle of the ventral segments. Directly the contrary will result from the friction of the string against the air.

Glass Works, near Birmingham.

XXIII. *On the Action of Solid Bodies on* [*Gaseous*] *Supersaturated Solutions.* By F. C. HENRICI[*].

NO. 52 of the *Naturforscher* (1869) gave a short account of numerous experiments by Mr. Tomlinson on the action of solids on supersaturated solutions, as brought before the Chemical Society. The views which Mr. Tomlinson has founded thereon, and which in their exposition called forth some objections, do not appear to me to follow from the phenomena; and as some experiments of my own strengthened my objections, I determined to investigate the matter fully. I have limited my numerous experiments to gas-impregnated water, a limitation which can have no injurious effect on the inquiry.

It is necessary first of all to define accurately what we mean by a supersaturated solution. If we consider how difficult it is to free water from air, even by boiling or by the action of the air-pump, there must evidently be between air and water an attraction, or so-called *adhesion*. The volume of air in solution must depend on this adhesion, and also on the atmospheric pressure at the time. The temperature has also considerable influence, since heat diminishes the adhesion and increases the expansive force of the air-molecules. There is also under the given conditions an equilibrium of pressure between the exterior and the dissolved air, which does not exist when these conditions are wanting. Hence, if the water contain too little air, it will absorb more, and in the opposite case part with it. In the latter case, therefore, the water is supersaturated with air, which is nearly always the case with spring-water. As respects gases which are not found in the atmosphere, or only in minute quantities (such as hydrogen, carbonic acid, and ammonia), the equilibrium of pressure does not obtain. Each of these gases, with respect to the quantity brought into contact with water, produces supersaturation more or less.

With respect to equality of pressure, the air particles in a gas-holding liquid are under the same conditions in all directions. If supersaturated, air particles continually escape from the surface, and others follow from the interior, their tendency to expand being thereby assisted. But this condition is entirely changed when the continuity of the liquid is interrupted by a solid body, as indeed it already is by the boundary walls of the containing

* Translated by Charles Tomlinson, F.R.S., from Poggendorff's *Annalen*, No. 12 (1872). The translator has made a number of critical remarks on this interesting paper, which he proposes to send in time for the April Number of the Philosophical Magazine. In the mean time he begs to refer to two papers by him on the same subject in the Philosophical Magazine for August and September 1867.

vessel. In such case, in addition to the attraction between the liquid and the air or gas, there is also an attraction between both of these and the solid sides; and it is a question which of these two attractions will prevail. There are three cases to be considered:—

1. The attraction between a liquid and air or gas has the preponderance; in this case the solid sides produce no change, and there is no separation of gas.

2. The attraction between the solid and the gas has the preponderance; in such case gas attaches itself to the solid, but does not escape.

3. The attraction between the solid and the liquid has the preponderance, in which case there is a condensation of gas on the solid surfaces, and, if the liquid is sufficiently supersaturated, an escape of gas (the quantity depending on circumstances).

The last case applies to Mr. Tomlinson's experiments, and also to my own. The first condition in such an inquiry is doubtless the most perfect cleanliness of the solid surfaces employed; for with unclean surfaces definite results cannot be obtained*. I have adopted the following method for obtaining clean surfaces— namely, rubbing them with fine pumice-stone powder sprinkled on soft leather. Surfaces so cleaned are well adapted to galvanic experiments; and their purity may be tested by the facility with which water wets them. The solids used were metal, glass, and bone : the supersaturated liquid, to begin with the simplest, was freshly drawn spring-water in small cylindrical glasses, and also in ordinary test-glasses. The observations were assisted by a double convex lens. The first experiments were with newly cleaned wires of different thicknesses of platinum, silver, brass, copper, plated copper, zinc, and steel, a strip of platinum, glass and bone rods. These were usually attached to a cork, and so sunk in the water to the depth of about two inches. No sooner were the solids in contact with the water, than they, as well as the side of the glass, were immediately dotted over with minute air-bubbles, which constantly increased in number and size until, after some time, the surfaces were more or less covered with them, occasionally quitting hold and ascending. No difference of action worth mentioning was to be observed between the different solids. With highly impregnated water, the bubbles reappeared more or less numerously next day on immersing the solids. These results were obtained, however often the experiments were repeated.

My next experiments were with water supersaturated with carbonic acid. The gas was generated by means of common effervescing powders, which enabled me to impregnate the water

* I have not made use of the terms " active " and " passive," since they are calculated to excite false ideas as to the mode of action.

to the required strength, and made me independent of the variations which constantly occur in spring-water*. By the use of small quantities of the powder and the before-mentioned solids immersed in the water, as soon as it became clear the action was very decided, so that decreasing quantities of the powder were sufficient to cause the solids to be covered with innumerable bubbles as soon as they were immersed. A fuller impregnation of the water produced a lively effervescence. The results are so beautiful as to leave nothing to be desired. Moreover the carbonic acid remains a remarkably long time in the water. In less than two cubic inches of carbonic-acid water, brass and silver wires acted after twenty-four hours and produced an abundant separation of bubbles.

Since, by frequent repetitions of these experiments, the same results were always obtained, and as I had bestowed the greatest care on the cleansing of the submerged solids, I am fully convinced that solids made perfectly clean, coming into contact with gas-impregnated water, is a necessary condition for the unequivocal production of the phenomena in question. This is in direct opposition to the hitherto received view, according to which *unclean*, and especially dusty surfaces, produce the separation of the bubbles in question. This view does not seem to have been submitted to rigid proof, but rather to have been supported by a solitary experiment. If a given surface, such as a silver wire, a glass rod, &c., be submitted to the action of a small flame of spirits of wine, such a surface will scarcely act, or not act at all, in separating gas. Hence it has been concluded that all kinds of organic substances contracted by exposure to the air were thus burnt off from the surface so as to leave it perfectly clean. This conclusion, however, follows so little from the premises, that the flame produces only a change in the covering and converts it into carbon or ash. This consideration has led me to inquire further into the matter; and the following are the results :—

A carefully cleaned glass rod was for some time moved over a small flame of spirits of wine, after which nothing could be seen upon its surface; but when it was drawn with slight pressure between the folds of a clean linen cloth, a frictional impediment was plainly felt, even producing a faint noise when the pressure

* These changes are really very great. My experiments with spring-water were for the most part carried on in summer; and I was very much surprised, on resuming them in autumn, to find the same water of no use. In summer the fermentation of the moist constituents of the soil is most active; we might therefore presume that the gaseous contents of spring-water at that season consist chiefly of carbonic acid; only the water drawn in autumn gave a precipitate with baryta-water. The solid contents of the water of this place are exceedingly small.

was increased, and the film could only be removed from the surface by a tolerably strong pressure.

The same effect was produced in the case of all kinds of metal. A clean silver teaspoon had its hollow held over the flame, after which this inner surface had no particular appearance, and became wetted with water much in the same way as the simply rubbed surfaces; nevertheless it cost me trouble to get rid of the frictional impediment in wiping out the hollow.

At the extremity of a strip of milk-white glass which had been held in the small flame, there could be seen, with the assistance of a lens, minute specks which could only be removed by very strong pressure.

We must also refer to the deposit of carbon on the bottom of flasks &c. heated by a spirit-flame.

Hence I must confidently conclude that, by the action of a spirit-flame, glass and other solids become covered with a scarcely recognizable (carbon?) film, which is itself sufficient to prevent the separation of gas-bubbles from aërated water, probably by absorption. The dust-particles which cover all bodies exposed to the air are most readily removed by rubbing with a clean duster, or rinsing with clean water. Surfaces which after previous cleaning are long exposed to the air and then so treated, act in fact like freshly cleaned ones. A glass rod so treated, a silver wire which had stood twenty-four hours and upwards in water, were covered abundantly with bubbles when put into aërated supersaturated water. The most convincing proof is the action of a pure quicksilver surface. In a small glass cylinder 5 centims. in height and 1¾ in width, a drop of pure mercury was poured sufficient to cover the bottom, and carbonic-acid water was carefully agitated with it. The mercury was immediately covered with rapidly swelling bubbles, which ascended and others formed in their place. As by the shaking of the glass the bubbles escaped from the mercury, this was immediately covered with new ones; and this result, notwithstanding the small quantity of gas in solution, could be repeated many times with scarcely any diminution, even in but slightly impregnated water, in which other surfaces did not act—thus showing that mercury, separating numerous bubbles, possesses a surpassing activity. Pure mercury forms indeed the most perfect surface that can be used; and it is completely wetted by water.

The choice of solids for these researches is somewhat limited. Thus pure metallic surfaces cannot be compared with oxidized ones, since the mechanical condition of the surface has the most powerful influence on the results, and is often entirely different in the two cases. If a clean polished brass wire be compared with one that has been well rubbed with sand paper, the latter

will be found to contain minute furrows, and if plunged into
aërated water will be immediately covered with innumerable
scarcely visible bubbles which arrange themselves in the furrows;
while on the first wire less numerous and, on that account, more
quickly growing bubbles appear. In the same way behaves a
strongly oxidized zinc wire that has long been exposed to the
air, the minute roughnesses of which are easily detected by rub-
bing with the finger; only in this case the furrows are absent.
An oxidized brass wire, as compared with one free from oxide,
shows, on the contrary, no great difference.

It has been already remarked that the temperature has a great
influence on the quantity of gas held in solution. If of two
volumes of equally impregnated water one be warmed, there is
a diminution of attraction between the air and water particles, and
also in the expansive force of both; supersaturation sets in; and if
this already existed, it is increased. Hence in air- or carbonic-
acid-impregnated water, it may happen that no bubbles can be
separated, but if gently heated by a small flame, or a warm metal
plate, the action sets in at once. By this simple means soda-water
or spring-water that has long been exposed to the air and appa-
rently of no further use, may again be brought into activity. In
water kept until the next day, in which no bubbles can be sepa-
rated (as is almost always the case), a gentle heating causes them
to appear anew and swell out in large numbers.

The experiments with oxidized zinc wire and with scratched
brass wire plainly show how important is the mechanical con-
dition of the surface on the liberation of gas. It was very
interesting to notice the action of the parts of these sap-containing
plants the surface of which is slightly rough, such as the stems
of the strawberry, milfoil or common yarrow, birch leaves with
their peduncles, &c. When these were immersed in the air- or
carbonic-acid-impregnated water, their minute projecting points
were quickly covered with numerous bubbles, which took up an
enormous quantity. The phenomenon was very interesting in
water weakly impregnated with air and carbonic acid, when this
was gently warmed. As the bubbles gradually formed, they were
from the first easily seen; the smallest appeared on the finest
hairs of the pine or Scotch fir, which were entirely covered with
them, and in sunshine displayed a brilliant show of colours.

These oft-repeated experiments convinced me that the air-
bubbles at the moment of their appearance are so small as to
be invisible, and by slow growth attain a visible size. How the
cohesion of the liquid at any given point within it (as is required
by the origin of a bubble) is overcome, cannot be made a matter
of observation; but that this takes place by means of the
slight roughnesses of the surface, fine points, &c., cannot, ac-

cording to my experiment, be doubted. Once admit the origin of a bubble, an enclosing liquid surface is thus created, into the hollow space of which other bubbles can penetrate, as continually happens at the free outer surface of the liquid, where the air particles can follow the bent of their force of expansion upwards.

The bubbles have, as a rule, so far as I have been able to make out, a spherical form; but they assume, in consequence of rapid growth, especially when thick together, an elongated form. In this case I have not remarked that they coalesce (a plain proof of the tenacity of the boundary walls), but they rather, when they have attained a certain size, become detached and ascend. Nor are the bubbles in actual contact with the operating surfaces, which may be accounted for by the adhesion of the liquid to the wet surfaces, whereby the fluid particles lose to a great extent their mobility; and hence it is intelligible why the bubbles cling with such remarkable tenacity to the surfaces: the smaller ones can scarcely be loosened by striking the vessel, and it is not till they have attained a certain size that they acquire ascensional force sufficient to detach them; so that on perpendicular or oblique surfaces they often ascend with diminished speed*. The nature of the surface seems from these results to have an influence. The bubbles do not attain so large a size on glass as on metal; the attraction between glass and water must be less than between metal and water, and hence the water particles would have less adhesion to glass.

If, as I doubt not, the condensation of the air-impregnated water on the immersed surfaces is the cause of the separation of the bubbles, such condensation acting in another way will produce the same result. The following experiments were made with water so slightly impregnated that an immersed silver wire produced no bubbles; and these experiments illustrate the well-known fact, that aqueous solutions under like conditions do not take up so much gas as pure water. By adding a little sulphuric acid, or portions of concentrated solutions of different readily soluble salts, to the impregnated water, there was a greater or less separation of gas-bubbles on the glass rod or other immersed surface. The following form of experiment gave the most striking results :—A copper wire hanging in a test-glass was twisted at the centre so as to hold some fibres of linen for the reception of the body that was to enter into solution and to retain it at the surface level of the liquid in the

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* Hence doubtless arises the remarkable phenomenon which I have observed in using the effervescing powders; namely, the ascending columns of the smallest bubbles were continually reflected from the under side of the upper surface, while many single ascending bubbles bounded repeatedly therefrom ere they could break through the thin surface.

test-glass. Fragments of carbonate of soda, saltpetre, common salt, &c. descended in concentrated streams visible to the eye. During this descent and before the solution had time to accumulate at the bottom of the jar and diffuse through the liquid, numerous bubbles separated on introducing a glass rod or silver wire; and this continued long in action, although only very small fragments of the salt were used. On using caustic alkali on air-impregnated water, there arose an opaque cloud of the finest bubbles as the saline streams descended; and this cloud slowly ascended and disappeared. The further course of the experiment was as with the other salts. This experiment was tried with water that had been boiled; but it is needless to add that nothing followed.

The well-known phenomenon will be remembered, where an effervescent wine, in which, after standing some time, bubbles no longer rise, foams when the glass is struck or agitated, and slight condensations of gas appear on the surface. If a blow be struck on the side of the glass with a rod of wood, bubbles immediately appear at the point struck.

De Luc referred these phenomena to the film of air which covers all bodies exposed to the atmosphere. There certainly are cases in which, on immersing solids in air-impregnated water, bubbles are produced in this way—as, for example, when the surfaces are such as not to be wetted by the water. I have made many experiments with dry, green grass halms, slightly rough on the surface, which show this result. On immersing them in supersaturated gaseous solutions, they became covered with innumerable bubbles, especially in the more extended air-spaces, such as the fine furrows &c.; such air-spaces remain longer in water exposed to the air; while such water may not give a single bubble to a metal surface. The air is evidently derived from that which is carried down during the immersion of the halm. All porous bodies act in the same way when immersed in liquids. Ripe grass halms with smooth surfaces behave like solids generally; they produce only a few bubbles.

It would doubtless be very interesting to carry out an exhaustive experimental inquiry into the cases 1 and 2, since they do not involve insuperable difficulties. In the meanwhile I cannot help offering a few remarks. Let us begin with the second case, and consider the phenomena which arise when the attraction between a solid surface and the gas in an aqueous solution prevails. In this case the first effect can only be the condensation of the gases on such surface—the very opposite to a separation. The adhesive attraction is but a preliminary step to the chemical, with which a condensation of the less dense consti-

tuents is indeed always connected. Only in the case in which the gas in solution unites chemically with the solid so as to form a gaseous body, could there be a separation of gas-bubbles.

There are many bodies which have for gases a strong condensing force or power of absorption, such, for example, as charcoal for carbonic acid. But their great porosity and numerous points and roughnesses (which soon exhaust their absorbing powers) render them scarcely available for experiments of this kind. I have endeavoured to prepare a piece of charcoal by plunging it red-hot into previously boiled water and keeping it immersed some days with occasional heating. On plunging it into highly impregnated water, only a few solitary bubbles appeared on its surface, while a silver wire similarly immersed was completely covered with them. The graphite from a common lead pencil or roughly dressed strips of the same material prepared as above, and plunged into the same highly impregnated solution of carbonic acid, produced no bubbles, notwithstanding the roughness of the surface. The non-appearance of the bubbles can only be ascribed to the condensation of the gas within the pores of the graphite.

I have prepared the charcoal in another way, namely by triturating it while thoroughly wet. The wet powder was then shaken into a test-glass; and after standing some days, the supernatant water was drawn off and soda-water carefully poured upon it. The result was as before, scarcely a bubble was to be seen; while in another test-glass, which instead of charcoal-powder contained the finest quartz sand, numerous bubbles appeared on it; and these being released by shaking, others took their place. Pieces of stone-coal with brilliant surfaces and one piece with polished sides were covered with bubbles.

The remarkable condensation of the mixed gases on a clean platinum surface, the condensation of oxygen alone, and even the less marked condensation of hydrogen on such a surface, may be made the subject of experiments with water sufficiently impregnated with these gases. A small glass cylinder 9 centims. in height and $1\frac{1}{2}$ centim. in width, was one evening filled with boiled water, and five or six drops of strong sulphuric acid added; a strip of zinc was then put in and the glass left in a cool place. The gas came off so quietly that on the following day it was still escaping. What was left of the zinc was taken out, and a well-cleaned platinum and silver wire inserted. The platinum had scarcely any action; but numerous bubbles appeared on the silver wire, the number and size of which increased, especially after warming the glass, while on the platinum wire not a bubble was to be seen.

For the investigation of the first case (preponderance of attrac-

tion between water and gas) a sufficient number of examples may be found. In an experiment with a strong solution of ammoniacal gas, in which a silver wire and a strip of platinum were immersed, no bubbles appeared on either surface, although innumerable bubbles were set free by the action of heat. Strong hydrochloric acid behaved in the same way with platinum; only on heating it more strongly fewer bubbles were produced, showing the much greater attraction between water and hydrochloric acid gas. By long exposure to the air both solutions became much weaker; and on gently warming them, the immersed wires became covered with bubbles, arising doubtless from the liquid having absorbed atmospheric air.

In conclusion, some experiments on fatty bodies may be referred to. Large drops of olive-oil, oil of almonds, and linseed-oil on the surface of water well impregnated with carbonic acid produced no separation of bubbles. The attraction of these oils for water is therefore not sufficiently energetic for the purpose; and it remains to be seen whether they absorb the gas to a slight extent. Stearic acid melted at the bottom of a small glass cylinder, and after becoming solid covered with soda-water, gave off an extraordinary display of bubbles. After some time the stearine loosened its hold and rose to the surface, separating from the fluid fat, a chemical action having taken place. With water previously boiled no bubbles were produced.

Freiburg, im Breisgau, January 1871.

XXIV. *On Arithmetical Irrationality.*
By J. W. L. Glaisher, *Fellow of Trinity College, Cambridge*[*].

IT is rather curious that, although very many of the numerical quantities with which the mathematician is constantly concerned are generally believed to be irrational (*i. e.* not to terminate or circulate[†] when expressed as decimals), yet the fact of such irrationality has been demonstrated in only a few cases; and it is still more remarkable that so little attention seems to have been given to the matter at all. To take an example, I suppose no one has any doubt (using the words in the sense that there is no one who would not be very much surprised if the contrary were proved) that all the sines in an ordinary Briggian or hyperbolic logarithmic canon, in which the arguments

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[*] Communicated by the Author.

[†] In the rest of this paper the word *circulate* will be supposed to include the case where the decimal terminates, as is indeed the fact; for a terminating decimal is merely one in which the circulating period consists entirely of zeros. Thus every numerical quantity either circulates and is rational, or does not circulate and is irrational.

are commensurable with a right angle, are irrational; but, as far as I know, no one has attempted to prove this; and the same may be said of many similar properties.

The most general theorems of this class with which I am acquainted were proved by Lambert in the Berlin Memoirs for 1761; he has there shown that the tangent of every rational arc (viz. arc commensurable with the unit arc, equal to radius) is irrational, and that the tangents of all angles commensurable with a right angle, except tan 45°, are irrational. From the first of these results it follows that π is irrational (since $\tan \frac{\pi}{4} = 1$); and to establish this was the main object Lambert had in view in undertaking his investigation. It also follows, though not quite so simply, that π^2 is irrational; but I believe no one has ever shown that π^3 or any higher power of π is so too; so that, as far as proof is concerned, π might be the nth root of a rational quantity, though, as Legendre has remarked, there is very little doubt that it is not the root of any algebraical equation with rational coefficients.

Lambert's principle consists in developing the quantity which is to be proved irrational into a continued fraction; and his result, stated in what is apparently the most generalized form it admits of, viz. that given to it by Legendre in the notes to his 'Geometry,' is that if in the continued fraction

$$\frac{\beta_1}{\alpha_1 +} \frac{\beta_2}{\alpha_2 +} \frac{\beta_3}{\alpha_3 +} \&c.$$

(extended to infinity) $\frac{\beta_1}{\alpha_1}, \frac{\beta_2}{\alpha_2} \ldots$, regarded as fractions ($\alpha_1, \alpha_2, \ldots \beta_1, \beta_2, \ldots$ all integers), be all less than unity, then, whether $\beta_1, \beta_2, \ldots$ be all positive or all negative, or some positive and some negative, the value of the continued fraction is irrational.

It is clear that the expansion of the quantity into a continued fraction is the most natural way of attacking the question, as the process is identical in principle with that of finding the greatest common measure in arithmetic.

Besides the theorems cited above with regard to tangents, Lambert showed that the hyperbolic logarithm (viz. logarithm to base $2.7182818\ldots$) of every rational number was irrational; and the corresponding theorem when the base of the system is a rational number is evident; for, to take the common base, we see at sight that $10^x = N$ (an integer) can only be satisfied by a rational value of x when N is a power of 10.

There is another method by which the irrationality of a series can be proved; but it is of exceedingly limited application. I

refer to the way in which e is usually shown to be irrational, viz. that if $e = \dfrac{n}{m}$ (m and n integers), then

$$\frac{m}{n} = \frac{1}{1 \cdot 2} - \frac{1}{1 \cdot 2 \cdot 3} \cdots \pm \frac{1}{1 \cdot 2 \ldots n} \mp \frac{1}{1 \cdot 2 \ldots (n+1)} + \ldots,$$

so that

$$m(1 \cdot 2 \ldots n-1) = \text{integer} \mp \left(\frac{1}{n+1} - \frac{1}{(n+1)(n+2)} + \ldots \right),$$

or integer = integer + fraction ;

so that m and n cannot be finite integers.

This method can only be applied when the denominators involve all numbers (or multiples of them), and when each denominator includes all the preceding ones, the numerators being constant.

There cannot be much doubt that in an ordinary natural canon sin 30° is the only rational sine, though I believe this has not been proved; ($\sin \dfrac{1}{x}$ and $\cos \dfrac{1}{x}$ are easily seen to be irrational; for x an integer, the unit being the arc equal to radius, by the sort of reasoning applied above to e^{-1};) and many other constants are in the same state, as, *e. g.*, $\log \pi$, e^{π}, &c., or Euler's constant ·577215 . . . .

Lambert's principle will be found to be not often applicable, as the conditions requisite are but seldom fulfilled; it is far more common for $\dfrac{\beta_1 \beta_2 \cdots}{\alpha_1 \alpha_2 \cdots}$ to be infinite than zero.

The method of proving the irrationality of certain quantities which I now proceed to explain, although very simple, does not seem to have been noticed; at all events Eisenstein, who was occupied with the irrationality of some of the quantities to which it is directly applicable, certainly did not perceive it. An example will make the principle of the method clear. Consider the series $1 + q + q^4 + q^9 + q^{16} + \ldots$ which occurs in the theory of Elliptic Functions; it follows at once that the value of this series is irrational whenever q is the reciprocal of an integer greater than unity; for if $q = \dfrac{1}{r}$, then in the scale of radix r the value of the series would be written

$$1 \cdot 1001000010000001000000001 \ldots,$$

which, as the intervals between the 1's consist of 2, 4, 6, 8, 10 . . . ciphers, does not circulate. In the scale of radix r, therefore, the series cannot be expressed in the form $\dfrac{M}{N}$, M and N being

finite integers. And as M and N are not integers in the scale of radix r, neither are they integers in the scale of radix 10, as a number expressible as an integer in one scale must clearly also be so expressible in any other scale, both the radixes being integers.

It is evident that the same kind of reasoning applies to all series of the form $1 + q^{\phi(1)} + q^{\phi(2)} + q^{\phi(n)} + \ldots$, where $\phi(n)$ is a rational non-linear function of n such that, when n is an integer, $\phi(n)$ is so too; so that all such series are irrational when q is the reciprocal of an integer greater than unity. We see also that the method is susceptible of being still further generalized, and gives a result in a great number of cases where the coefficients are not unity nor all positive; thus

$$1 - q + q^4 - q^9 + \ldots = \cdot 900099999000000099 \ldots$$

(q being, as throughout the rest of this paper, $\dfrac{1}{r}$, and the symbol 9 denoting the digit $r-1$) cannot circulate; and the same is the case with $1 + q + 2q^4 + 3q^9 + \ldots$, $1 + q + 4q^4 + 9q^9 + \ldots$, &c., and generally with $1 + \psi(1)q + \psi(2)q^4 + \psi(3)q^9 + \ldots$, where $\psi(n)$ is such that the number of figures constituting it in the scale of radix r always bears a ratio less than unity to $2n-1$ (which is the difference between the number of ciphers preceding the first significant figures in $q^{(n-1)^2}$ and q^{n^2}),—as then the numbers of figures in the groups of 0's or 9's continually increase, so that the decimal cannot circulate.

The number of figures that any number u will occupy when expressed in the scale of radix r is equal to the integer next above $\log_r u$; so that if we apply the above reasoning to the series $1 + xq + x^2q^4 + x^3q^9 + \ldots$, we see that so long as x is an integer such that $\log_r x^{n+1} < 2n$ (that is, if x be an integer less than r^2), its value is irrational.

That the number of zeros in a group is in this case ultimately infinite is apparent, as the number in question when $x = r^2 - 1$ is approximately $2n - \log_r(r^2-1)^{n+1}$, which

$$= (n+1)\log_r \frac{r^2}{r^2-1} - 2,$$

and is infinite with n; that the series is irrational when $x = r^2$ is easily seen otherwise.

The most general case to which the method is applicable is that of the series $1 + \psi(1)q^{\phi(1)} + \psi(2)q^{\phi(2)} + \psi(3)q^{\phi(3)} + \ldots$, which is irrational if $\log_r \psi(n) + 1 < \phi(n) - \phi(n-1)$, so that when n is infinite, $\phi(n) - \phi(n-1) - \psi(n)$ is infinite too,—$\phi(n)$ being as above, and $\psi(n)$ a rational function of n, which is an integer when n is an integer.

It is not essential that the exponents of the powers of q should be given by an algebraical formula $\phi(n)$; the reasoning is equally successful when they are the series of prime numbers, or of their squares, cubes, &c.

I need scarcely remark that the sole condition for the success of the method is that the groups of 0's or 9's do not circulate; so long as this is the case the signs $\pm$ may occur in the terms in any order. A good example is afforded by the product $(1-q)(1-q^2)(1-q^3)\ldots$, which Euler proved to be equal to

$$1-q-q^2+q^5+q^7-q^{12}-\ldots,$$

the general term being $(-)^n q^{\frac{1}{2}(3n^2 \pm n)}$, so that the product, when q is the reciprocal of an integer, is evidently irrational.

It will thus have been seen that the method is applicable to a considerable number of series, the irrationality of which is not seen otherwise. Lambert's principle, however, can be applied to a good many cases by means of the following formulæ:—

$$\frac{1}{a_1} - \frac{1}{a_1 a_2} + \frac{1}{a_1 a_2 a_3} - \ldots = \cfrac{1}{a_1 + \cfrac{a_1}{a_2-1+ \cfrac{a_2}{a_3-1+ \&c.}}}, \quad (1)$$

$$\frac{1}{a_1} + \frac{1}{a_1 a_2} + \frac{1}{a_1 a_2 a_3} + \ldots = \cfrac{1}{a_1 - \cfrac{a_1}{a_2+1- \cfrac{a_2}{a_3+1+ \&c.}}}, \quad (2)$$

which are proved at once, since

$$\frac{1}{a_1} - \frac{1}{a_1 a_2} = \cfrac{1}{a_1 + \cfrac{a_1}{a_2-1}},$$

so that the nth convergent to the fraction is identically equal to the first n terms of the series.

From (2) we have

$$1 + \frac{1}{r^\alpha} + \frac{1}{r^\beta} + \frac{1}{r^\gamma} + \ldots 1 + \cfrac{1}{r^\alpha - \cfrac{r^\alpha}{r^{\beta-\alpha}+1- \cfrac{r^{\beta-\alpha}}{r^{\gamma-\beta}+1-\&c.}}} \quad ; \quad (3)$$

since $r^\beta = r^\alpha . r^{\beta-\alpha}, \quad r^\gamma = r^\alpha . r^{\beta-\alpha} . r^{\gamma-\beta},$ &c.;

so that if $\alpha, \beta, \gamma, \ldots$ are such that the differences $\beta-\alpha, \gamma-\beta, \ldots$ after some point always increase, Lambert's principle is applicable and the series is irrational—the same result as was obtained above when the series was written $1 + q^{\phi(1)} + q^{\phi(2)} + \ldots$ The same reasoning also applies when any of the signs are negative;

but this method either does not succeed at all, or if it does, the process is not so convenient, when there are coefficients $\psi(1)$, $\psi(2)$, &c. to the powers of q. Applying (3) to the series $1 + q + q^4 + \ldots$, we have

$$1 + \frac{1}{r} + \frac{1}{r^4} + \frac{1}{r^9} + \ldots = 1 + \cfrac{1}{r - \cfrac{r}{r^3 + 1 - \cfrac{r^3}{r^5 + 1 - \cfrac{r^5}{r^7 + 1 - \&c.}}}} \qquad . \quad (4)$$

This, and the corresponding fraction when the alternate terms of the series are negative, was given by Eisenstein in vol. xxvii. p. 193 of Crelle's *Journal*, where he has applied it to prove the irrationality of the series. He has also proved by means of a continued fraction a more general theorem, viz. that

$$1 + \frac{x}{r} + \frac{x^2}{r^4} + \frac{x^3}{r^9} + \ldots$$

is irrational whenever r is an integer, and x a rational fraction not greater than unity, while it has been shown above that the series is irrational if x is an integer not greater than r^2; so that the methods give results which, though they overlap, do not coincide. Eisenstein has not stated how he obtained the fraction (4); but the manner in which he has stated his result leads to the inference that it was by means of treating the series

$$\frac{1}{r} + \frac{1}{r^4} + \frac{1}{r^9} + \ldots \quad \text{and} \quad 1 + \frac{1}{r} + \frac{1}{r^4} + \frac{1}{r^9} + \ldots$$

in a manner analogous to that of finding their greatest common measure.

In a letter to Jacobi (Crelle, vol. xxxii. p. 205), Heine has proved Eisenstein's theorem (4) by transformations from Euler's formula

$$a - b + c - d + e - \ldots = \cfrac{a}{1 + \cfrac{b}{a - b + \cfrac{ac}{b - c + \cfrac{bd}{c - d + \&c.}}}},$$

which really comes to the same thing as using (2), although the work is much longer, reductions &c. being required.

I may also mention that the usual way of treating series of the same form as that in (3), viz. by means of the formula

$$\frac{1}{b_1} - \frac{1}{b_2} + \frac{1}{b_3} - \ldots = \cfrac{1}{b_1 + \cfrac{b_1^2}{b_2 - b_1 + \cfrac{b_2^2}{b_3 - b_2 + \&c.}}},$$

gives

$$1 - \frac{1}{r^\alpha} + \frac{1}{r^\beta} - \ldots = 1 + \cfrac{1}{r^\alpha + \cfrac{r^{2\alpha}}{r^\beta - r^\alpha + \cfrac{r^{2\beta}}{r^\gamma - r^\beta + \&c.}}},$$

which on dividing out superfluous powers of r gives the same form as (3).

The product $\dfrac{(1-q^2)(1-q^4)(1-q^6)\ldots}{(1-q)(1-q^3)(1-q^5)\ldots}$ is known to be equal to $1 + q + q^3 + q^6 + q^{10} + \ldots$ (the exponents being the triangular numbers), and this latter series Eisenstein (Crelle, vol. xxix. p. 96) developed into the fraction

$$\cfrac{1}{1 - \cfrac{1}{r - \cfrac{1-r}{r - \cfrac{1}{r^2 - \cfrac{1-r^2}{r^2 - \&c.}}}}}$$

$(q = \frac{1}{r})$, whence (in the British-Association Report, 1871, Trans. Sect. p. 16) I inferred its irrationality by means of Lambert's principle. The irrationality is evident at sight by the principle explained in this paper; also (3) gives us the means of expanding the series into another continued fraction, to which also Lambert's method applies, viz.

$$1 + \frac{1}{r} + \frac{1}{r^3} + \frac{1}{r^6} + \ldots, = 1 + \cfrac{1}{r - \cfrac{r}{r^2 + 1 - \cfrac{r^2}{r^3 + 1 - \cfrac{r^5}{r^4 + \&c.}}}}.$$

In another memoir (Crelle, vol. xxviii. p. 39) Eisenstein has developed Euler's product into a continued fraction as follows:

$$\left(1 - \frac{1}{r}\right)\left(1 - \frac{1}{r^2}\right)\left(1 - \frac{1}{r^3}\right)\ldots = 1 + \cfrac{1}{1 - r - \cfrac{1}{1 + r + \cfrac{r^2}{1 - r^3 - \cfrac{r}{1 + r^2 - \&c.}}}}$$

(the alternate denominators being $1-r$, $1-r^3$, $1-r^5$, $\ldots$ and $1+r$, $1+r^2$, $1+r^3$, $\ldots$, and the corresponding numerators 1, r^2, $r^4 \ldots$ and 1, r, $r^2 \ldots$), whence he has inferred its irrationality. Formula (1), however, affords the means of obtaining a still simpler form for the product in question as a continued fraction; for by a well-known theorem,

$$\left(1 - \frac{1}{r}\right)\left(1 - \frac{1}{r^2}\right)\left(1 - \frac{1}{r^3}\right)\ldots = 1 - \frac{1}{r-1} + \frac{1}{(r-1)(r^2-1)}$$
$$- \frac{1}{(r-1)(r^2-1)(r^3-1)} + \ldots,$$

which by (1)

$$= \cfrac{1}{r-1 + \cfrac{r-1}{r^2-2 + \cfrac{r^2-1}{r^3-2 + \&c.}}},$$

to which also Lambert's principle applies.

I may mention that Eisenstein (whose chief results have been incidentally reproduced above) has enunciated his theorems without demonstration, and evidently intended to return to the subject, though after an examination of his subsequent memoirs I feel pretty certain he never did do so. He states that he was in possession of a more general theorem; and I think it likely that the result he alludes to was that marked (3) above, or some other continued fraction of kindred scope, from which the irrationality of a good many series could be deduced. Eisenstein must have been aware of the irrationality of $1 + q + q^3 + q^6 + \ldots$, as appears from his theorems with reference to series of a similar kind given two years previously; but, curiously enough, he has omitted to state it as a conclusion to be deduced from the continued fraction into which he transformed it; and at the time of writing the paper of which the abstract appeared in the British-Association Report, I had not seen his previous papers, and in fact did not know he had considered the subject of irrationality at all; so that I drew the inference which it was probably merely an accidental omission that Eisenstein had not himself pointed out. The method explained in this paper, however, is far more simple and appropriate in such cases.

I may mention that Eisenstein also gave another continued fraction for the series with squares as exponents, viz.

$$1 + \frac{1}{r} + \frac{1}{r^4} + \frac{1}{r^9} + \ldots \cfrac{1}{1 - \cfrac{1-r}{r - \cfrac{1-r^2}{r^2 - \cfrac{1}{r^3 - \&c.}}}},$$

resembling that given by him for $1 + \dfrac{1}{r} + \dfrac{1}{r^3} + \dfrac{1}{r^6} + \ldots$

It is interesting, in conclusion, to note that the method explained above gives the proper result for the geometrical series $1 + q^\alpha + q^{2\alpha} + q^{3\alpha} + \ldots$, which would be written in the scale of radix r $1 \cdot 00 \ldots 100 \ldots 1$ ($\alpha - 1$ ciphers in each group), which *does* circulate, as of course it ought to do, since the sum is $\dfrac{1}{1 - q^\alpha}$.

Cambridge, February 11, 1873.

XXV. *On a Method of Testing Submarine Telegraph Cables during Paying-out. By* THOMAS T. P. BRUCE WARREN, *Electrician to Hooper's Telegraph Works, Limited; Member of the Society of Telegraph Engineers* *.

IT is a singular circumstance that although within the last few years we have become, as it were, inundated with new appliances for testing submarine telegraph-cables during their manufacture, so little has been effected towards the improvement of electrical testing during the paying-out.

Apart from the uncertainty which has lately been shown to exist in galvanometric measurements themselves, there are many little difficulties to encounter when testing on board ship, which at times are so embarrassing as to make one forego a very large share of confidence in the results. The electrical condition of the cable must consequently be a matter of great anxiety, until a steady observation enables the electrician to decide that every thing is all right.

The practice has hitherto been to make the duties at the shore station too subsidiary a matter in the system of testing, instead of relying upon the results obtained on shore as of primary importance. Considering the facilities offered when the instruments are perfectly at rest and consequently admit of much more accurate adjustment and indication than is possible on the ship, it is evident that we should look to the shore observations with an equal, if not greater interest.

In the present systems of testing, the shore operations have been usually so formulated that any separate or independent observation is quite inadmissible, and, unless distinctly and definitely preconcerted by the ship, would lead to serious results.

Any system of testing on the ship should be capable of being synchronously carried on at the shore station without impeding any modified operation required on the ship; and either the ship or shore should be capable of being made the controlling station, and in such a way as not to interfere with each other.

The electrician has to provide for two kinds of faults, one of which is brought about by the rupture of the conductor, and the other by a flaw or defect in the dielectric.

The rupture of the conductor involves the cessation of signals from one end to the other, and may be either accompanied by or without defective insulation on one or both sides of the broken conductor.

As the shore must transmit its results to the ship telegraphically, it is evident that in the case of "no continuity"† the ship

* Communicated by the Author.
† The technical expression for a broken conductor.

must follow out its own system of testing for the position of the rupture in the conductor; the only provision which can be made by the shore station, in helping the ship to determine the position of such a fault, is in sending " continuity "-signals as often as possible, so that the ship, at any time failing to receive them, can pronounce upon the position of the rupture without any loss of time.

The following system of " continuity "-signals, suggested by the author in an article which appeared in ' Engineering,' April 24, 1868, serves at the same time to inform the ship and shore station whether the conductor is perfect or broken.   The great advantage of this system is that it leaves the electrified condition of the cable undisturbed during the intervals of signalling.

The shore end of the cable being landed, the conductor, or a prolongation of it, is attached to an insulated pin; and at a suitable distance from it is fixed another insulated pin which is connected through a delicate galvanometer to earth; between these two pins an insulated pendulum is made to vibrate.

The pendulum on touching the pin connected with the cable, takes from it a certain proportion of its charge, and during its oscillation empties it into the pin which is connected to the galvanometer.

The capacity of the pendulum is first fixed upon for the length and electrostatic relation of the cable.   Increased capacity may be given to the pendulum by attaching to it a condenser of suitable electrical dimensions.

It will be sufficient to insulate from the other portions of the pendulum that part which is concerned in making the contacts.

On contact with the end of the cable by the pendulum, a momentary increase in the deflection will be observed on the galvanometer in the ship, arising from the sudden increase of capacity; and this will continue as a constant range of variation in the angular deflection due to leakage on the cable itself.   By means of these slight motions, a " test of continuity " is kept up without interfering with the test of insulation which is always kept on in the ship.   The observer on shore is enabled to know with positive certainty the electrical condition of the cable by allowing the abstracted charges to flow to earth through the galvanometer, occasionally noting the deflection.

If the conductor parted, and at the same time each end remained insulated, the observer at the shore end would continue to get indications on his galvanometer; but the intermittent rise and fall in the galvanometer deflection in the ship would immediately cease, and the engineer would know the exact moment that the continuity ceased.

The observer on shore would very soon observe, by the deflection on his galvanometer becoming gradually but very regularly lessened, that the conductor had parted, and the end on the shore side was enclosed in the insulator. If, on the other hand, the deflections suddenly ceased, he would further know that the conductor had parted and was exposed.

In practice it is found preferable, instead of automatically connecting the condenser with the cable and galvanometer, to use an ordinary charging and discharging key, and to perform this test at intervals of every five minutes. According to Mr. Latimer Clark (Electrical Measurement, &c.) this was carried out on the Atlantic cables in 1866, although it was perfectly unknown to the author at the time this article appeared.

In the present method it is proposed to obtain " continuity "-indications on the shore, and at the same time to measure the tensions of the end at the shore by means of Sir William Thomson's quadrant-electrometer.

Now, since the conductor of a cable may be parted with insulation still perfect, the electrometer should be capable of showing the fall of tension in 20 or 30 seconds on the highest degree of insulation; but an electrometer with this degree of sensibility could not be used in the ordinary way, when the cable is connected on the ship with 100, or any number of cells usually employed; for the tension of the cable at the shore end would repel the needle in one direction completely beyond the range of being read.

In order to obviate this, the end of the cable is to be connected to one pair of quadrants, and to the other pair a battery in all respects similar to the testing battery on the ship. If the tension of the ship's battery, as found at the shore end of the cable, be such as to balance the opposing battery on the shore, the electrometer will maintain its zero position; and if we could be sure of setting up any ordinary testing battery in two places and at different times alike, we should be able at once to obtain a most accurate comparison of the tensions at the two ends of the cable.

The shore-electrometer being used in this way, the slightest flaw which could not be possibly recognized on the ship's galvanometer would be easily detected, and in deep water a long time before it could possibly reach the bottom.

In this way, by one single observation, the shore obtains a continuity test, an insulation test, and at the same time, should any thing go wrong, the adjustment required to bring the electrometer-needle to its zero position supplies the data for localizing any defect which might arise. Should the ship wish to open communication with the shore, the same instrument is at once ready to receive a "call" signal, or a complete but simple message.

Not to rely solely upon the electrometer for the measurements of tensions, a modification of the condenser method previously described, and which will serve equally well to convey continuity-signals to the ship, will supply a check-measurement of these tensions, and in a way so as to give at once continuity-indication to the ship and the tension in volts to the shore observer.

The charge abstracted from the cable in one condenser is sent through a delicate galvanometer in one direction, simultaneously that a condenser of similar capacity, charged to the same tension from a constant battery, flows in the other direction; a current will be perceptible on the galvanometer flowing from one condenser to the other, should they not be charged to the same tension. The changes due to the galvanometer itself will not interfere; and thus not only will a considerable amount of irksome testing and calculating be avoided, but the results themselves be more accurate.

These tensions will be telegraphed to the ship at stated times. The tension of the ship's battery will be carefully measured on a delicate portable electrometer on board ship for comparison with the results sent from the shore station. The fall of tension on the cable can then be simultaneously noted on the shore as well as on the ship; and during this time the continuity-signals need not be interrupted, as the sensibility of the ship's electrometer will allow the small impulses to be noted, but, instead of in a current form, either by regular and equal abstractions of charge, or by the shore attendant at stated times restoring the full tension. This, in addition to forming a perfect continuity-signal, will at once indicate to the ship the loss observed on shore during any specified space of time. In the same way the ship may inform the shore observer; and both will be able to verify or compare their results.

Any difference of tension can thus be instantly communicated from shore to ship, or *vice versâ*, without the necessity of signalling the results as noted by the sliding resistances or by the condenser method; the signal itself will convey all the data required by the ship, should this difference of tension arise from the existence of a fault.

The reduction and comparison of these tensions should in every case be made with the constant cells of Mr. Latimer Clark. Now the ship and the shore doing this independently of each other, should still arrive at the same result, so long as every thing remains right; but in the event of a slight fault occurring, the two results will not coincide, and the difference observed will correspond to the magnitude of the fault; and as the precise tension at the fault can be measured from either end, the

difficulty should be considerably diminished in localizing small faults.

The balancing of the ship's battery on the shore-electrometer by a similar battery stationed on shore, should be done either with a set of ordinary resistance-coils, or, preferably, by a set of Sir William Thomson's resistance-slides.

The points for consideration are, that the battery which has the highest potential should be bridged over with a resistance sufficiently high, so that it may be impossible to produce any appreciable alteration whilst the test is being applied*. The same remark applies to the constant cells employed to balance the discharge from the condenser used for the continuity-signals; for this purpose, too, it will be useful occasionally to take the condenser-tensions on the quadrant-electrometer with the resistance-slide and constant cells in opposition. The insulation of the condensers should be so perfect that no loss can be perceptible during the time of making these tests; and they should also, on this account, be properly and completely charged, whether from the cable or batteries. When using the condensers with the ship's electrometer for continuity-signals to ship, it is equally important that they should be completely discharged. Any tendency in the condensers to give too great residual discharges should be carefully studied beforehand, and their effect on the portable electrometer well noted.

The constant cells used on the ship and on shore, and also the electrometers, should all be very carefully compared by testing against each other a short time before the paying-out commences.

The method here given for measuring the discharges from the condenser will serve as an efficient test for comparing batteries; it involves, however, that their resistances should be ascertained. A great advantage in favour of this plan is, that when the electromotive forces of the cells to be compared are nearly the same, the resistances with which the stronger cell is bridged over will be quite incapable of producing any alteration in its electromotive force.

Tamworth House,
Mitcham Common.

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* The battery on shore should contain a sufficient number of cells above that on the ship, so as to be capable of being balanced against any increase of tension from the ship's battery.

XXVI. *On the Pressure required to give Rotation to Rifled Pro-jectiles.* By Captain Noble, *F.R.S. &c. (late Royal Artillery)**.

[With a Plate.]

1. IN a paper published in the Philosophical Magazine for 1863 (vol. xxvi.), and subsequently in the *Revue de Technologie Militaire,* I gave some investigations on the ratio between the forces tending to produce translation and rotation in the bores of rifled guns.

2. My object in these investigations was to show, 1st, that in the rifled guns with which experiments were then being made the force required to give rotation was generally only a small fraction of that required to give translation ; 2ndly, that in all cases (and this was a point about which much discussion had taken place) the increment of gaseous pressure (that is, the in-crease of bursting force) due to rifling was quite insignificant.

3. In the paper referred to, although the formulæ were suffici-ently general to embrace the various systems of rifling then under consideration in England, they did not include the case of an increasing twist, which has since been adopted for the 8-inch and all larger guns of the British service ; neither was our knowledge of the pressure of fired gunpowder sufficient to enable me to place absolute values on either of the forces I was considering.

4. Since the date at which I wrote, an extensive series of expe-riments has been made in this country ; and the results of these experiments enable me to give with very considerable accuracy both the pressure acting on the base of the projectile and the velocity at any point of the bore. I am therefore now able not only to assign absolute values where in my former paper I only gave ratios, but also to show the amount by which the studs of the projectiles of heavy guns have been relieved by the introduction of the accelerating twist known as the parabolic system of rifling.

5. Very little consideration will satisfy any one conversant with the subject, that in the ordinary uniform spiral or twist the pressure on the studs or other driving-surface is a maximum when the pressure on the base of the shot is a maximum, and becomes greatly reduced during the passage of the shot from its seat to the muzzle of the gun. In my former paper I showed, in fact, that in a uniform twist the pressure on the studs was a con-stant fraction of the pressure on the base of the shot, the value of the fraction of course depending on the angle of the rifling ; and as it is evident that the tension of the powder-gases at the muzzle is very small when compared with the tension of the same gases

* Communicated by the Author.

PRESSURE ON STUDS

(R.L.G.)An uniform spiral of 1 turn in 40 Calibres.

(Pebble)An uniform spiral of 1 turn in 40 Calibres.

(Pebble) An uniformly increasing twist of 1 turn in 100 Calibres at Breech.
to 1 turn in 40 Calibres at Muzzle.
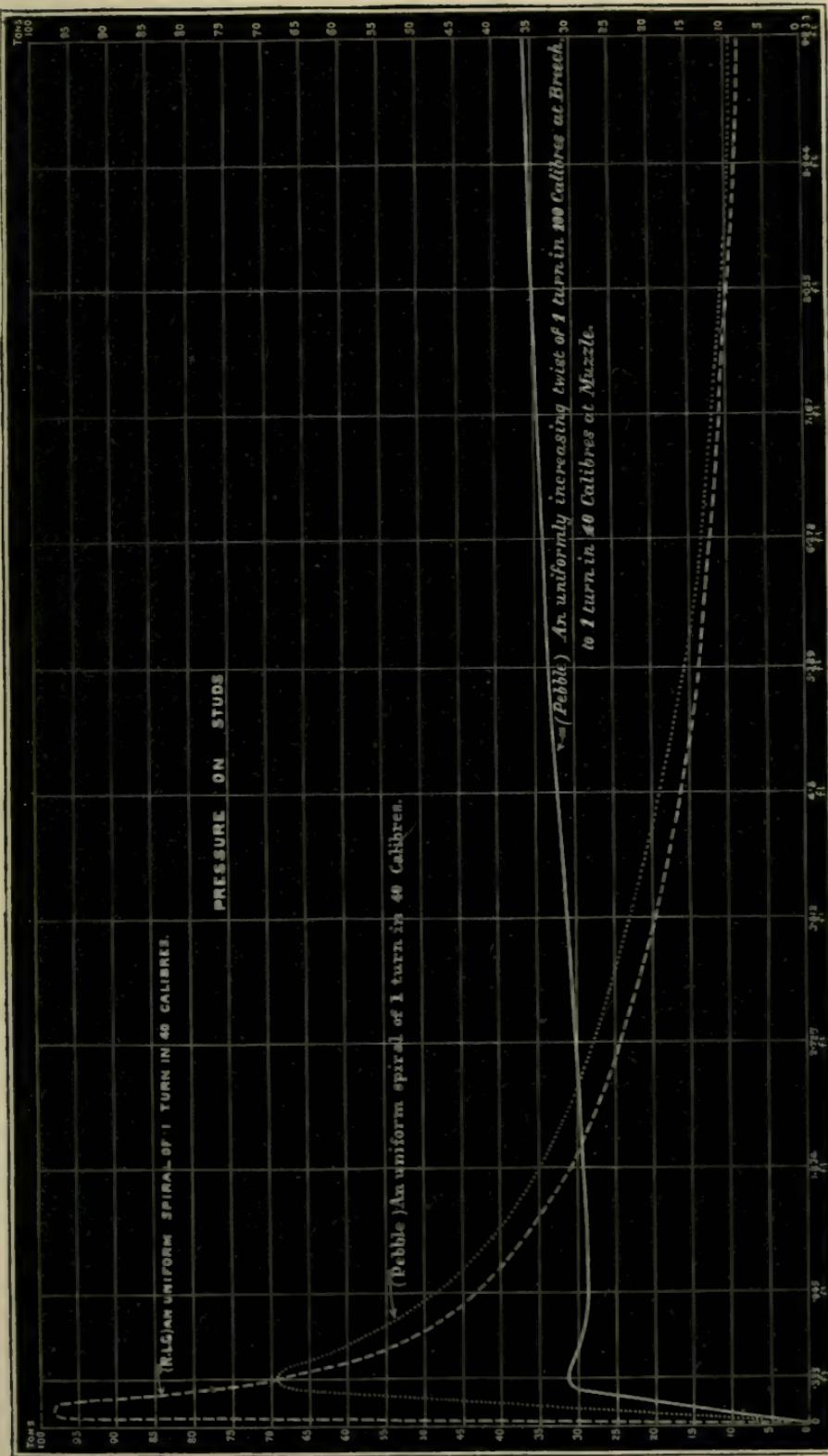
mence at zero, it will be found more convenient to make the
plane of xy pass through the point where the twist would be
zero were the grooves sufficiently prolonged. Let the axis of x
pass through one of the grooves; and, for the sake of simplicity, we
shall suppose the rifling to be given by one groove only. Let the
axis of z be coincident with that of the gun; let A P (see fig. 1)

be the groove or curve described by the
point P, and let P (x, y, z) be the point
at which the resultant of all the pressures
tending to produce rotation may be as-
sumed to act at a given instant. Let the
angle A O N $= \phi$.

Fig. 1.

11. Now the projectile in its passage
through the bore is acted on by the fol-
lowing forces:—

1st. The gaseous pressure G, the re-
sultant of which acts along the axis of z.

2nd. The pressure tending to produce
rotation. Calling this pressure R, and
observing that it will be exerted normally
to the surface of the groove, we have for
the resolved parts of this pressure along
the coordinate axes, R $\cos \lambda$, R $\cos \mu$, and R $\cos \nu$—λ, μ, and ν
being the angles which the normal makes with the coordinate
axes.

3rd. The friction between the stud or rib of the projectile
and the driving-surface of the groove. This force tends to re-
tard the motion of the projectile; its direction will be along the
tangent to the curve which the point P describes. If μ_1 be the
coefficient of friction, and if α, β, γ be the angles which the tan-
gent makes with the coordinate axes, the resolved portions of
this force are μ_1R $. \cos \alpha$, μ_1R $. \cos \beta$, μ_1R $. \cos \gamma$.

12. Summing up these forces, the forces which act

$$
\left.
\begin{array}{llll}
\text{parallel to } x \text{ are } & X = R . \{\cos \lambda - \mu_1 \cos \alpha\}, \\
\quad\quad\quad ,, \quad\quad y \quad ,, & Y = R . \{\cos \mu - \mu_1 \cos \beta\}, \\
\quad\quad\quad ,, \quad\quad z \quad ,, & Z = G + R . \{\cos \nu - \mu_1 \cos \gamma\};
\end{array}
\right\} \quad . \quad (1)
$$

and the equations of motion are

$$
M . \frac{d^2 z}{dt^2} = G + R \{\cos \nu - \mu_1 \cos \gamma\}, \quad . \quad . \quad . \quad (2)
$$

$$
M . \frac{d^2 \phi}{dt^2} = \frac{Yx - Xy}{\rho^2}, \quad . \quad . \quad . \quad . \quad . \quad (3)
$$

ρ being the radius of gyration. Equations (1), (2), and (3) are
identical with those I formerly gave.

13. Now, in the case of a uniformly increasing twist, the
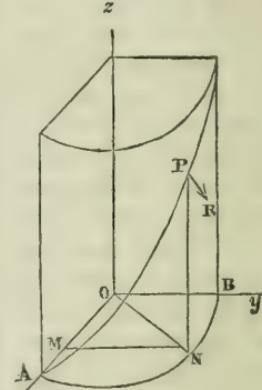
equations to the curve which when developed on a plane surface is a parabola may be put under the form

$$x = r \cos \phi; \quad y = r \sin \phi; \quad z^2 = kr\phi. \quad \cdot \quad \cdot \quad (4)$$

Hence

$$dx = -r \sin \phi . d\phi; \quad dy = r \cos \phi . d\phi;$$

$$dz = \frac{kr}{2z} . d\phi; \quad ds = \frac{r}{2z} \sqrt{4z^2 + k^2};$$

and we have, to determine the angles which the tangent to the curve described by P makes with the coordinate axes, the equations

$$\left.\begin{array}{l}
\cos a = \dfrac{dx}{ds} = \dfrac{-2z . \sin \phi}{\sqrt{4z^2 + k^2}}, \\[2mm]
\cos \beta = \dfrac{dy}{ds} = \dfrac{2z . \cos \phi}{\sqrt{4z^2 + k^2}}, \\[2mm]
\cos \gamma = \dfrac{dz}{ds} = \dfrac{k}{\sqrt{4z^2 + k^2}}.
\end{array}\right\} \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (5)$$

14. In the Woolwich guns the driving-surface of the groove may be taken, without sensible error, as the simpler form of surface where the normal to the driving-surface is perpendicular to the radius, the surface itself being generated by that radius of the bore which, passing perpendicularly through the axis of z, meets the curve described by the point P; but in the first instance I shall examine the more general case, where the normal makes any assigned angle with the radius.

Assume then that on the plane of xy the normal makes an angle δ with the radius of the gun. The driving-surface of the groove is then swept out by a straight line which, always remaining parallel to the plane of xy, intersects the curve described by P, and touches the right cylinder whose axis is coincident with that of z, and whose radius $= r . \cos \delta$.

Now, the equations to the director being given by (4), and that to the cylinder, which the generator always touches, being

$$x^2 + y^2 = (r \cos \delta)^2, \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (6)$$

it is easily shown that the coordinates x_1, y_1 of the point of contact of the tangent to the cylinder drawn from P parallel to the plane xy, are

$$\left.\begin{array}{l}
x_1 = r . \cos \delta . \cos (\phi - \delta), \\
y_1 = r . \cos \delta . \sin (\phi - \delta),
\end{array}\right\} \quad \cdot \quad \cdot \quad \cdot \quad (7)$$

and that the equation to the driving-surface is

$$x . \cos \left\{ \frac{z^2}{kr} - \delta \right\} + y . \sin \left\{ \frac{z^2}{kr} - \delta \right\} = r . \cos \delta. \quad \cdot \quad (8)$$

15. The angles which the normal to this surface make with the coordinate axes are given by

$$\cos \lambda = \frac{\left(\dfrac{dF}{dx}\right)}{\sqrt{\left(\dfrac{dF}{dx}\right)^2 + \left(\dfrac{dF}{dy}\right)^2 + \left(\dfrac{dF}{dz}\right)^2}},$$

with similar expressions for $\cos \mu$ and $\cos \nu$.   But

$$\left(\frac{dF}{dx}\right) = \cos\left(\frac{z^2}{kr} - \delta\right); \ \left(\frac{dF}{dy}\right) = \sin\left(\frac{z^2}{kr} - \delta\right); \ \left(\frac{dF}{dz}\right) = \frac{2z}{k}.\sin\delta\,;$$

$$\sqrt{\left(\frac{dF}{dx}\right)^2 + \left(\frac{dF}{dy}\right)^2 + \left(\frac{dF}{dz}\right)^2} = \frac{1}{k}\sqrt{4z^2(\sin\delta)^2 + k^2}.$$

Therefore the angles which the normal to the driving-surface makes with the axes are given by

$$\left.\begin{aligned}
\cos \lambda &= -\frac{k.\cos\left(\dfrac{z^2}{kr} - \delta\right)}{\sqrt{4z^2(\sin\delta)^2 + k^2}}, \\
\cos \mu &= -\frac{k.\sin\left(\dfrac{z^2}{kr} - \delta\right)}{\sqrt{4z^2(\sin\delta)^2 + k^2}}, \\
\cos \nu &= -\frac{2z.\sin\delta}{\sqrt{4z^2(\sin\delta)^2 + k^2}}.
\end{aligned}\right\} \quad \cdots \quad (9)$$

16. Substituting in (2) and (3) the values given for α, β, γ, λ, μ, ν in (5) and (9), the equations of motion become

$$M.\frac{d^2z}{dt^2} = G - R\left\{\frac{2z\sin\delta}{\sqrt{4z^2(\sin\delta)^2 + k^2}} + \frac{\mu_1 k}{\sqrt{4z^2 + k^2}}\right\}, \quad (10)$$

$$M.\frac{d^2\phi}{dt^2} = \frac{R.r}{\rho^2}\left\{\frac{k.\sin\delta}{\sqrt{4z^2(\sin\delta)^2 + k^2}} - \frac{2\mu_1 z}{\sqrt{4z^2 + k^2}}\right\}; \quad (11)$$

and from (11),

$$R = \frac{M.\rho^2}{r\left\{\dfrac{k.\sin\delta}{\sqrt{4z^2(\sin\delta)^2 + k^2}} - \dfrac{2\mu_1 z}{\sqrt{4z^2 + k^2}}\right\}}.\frac{d^2\phi}{dt^2}. \quad (12)$$

17. To determine $\dfrac{d^2\phi}{dt^2}$.

From (4), $\qquad kr\phi = z^2,$

$$\therefore \ kr \cdot \frac{d\phi}{dt} = 2z \cdot \frac{dz}{dt},$$

$$kr \cdot \frac{d^2\phi}{dt^2} = 2 \cdot \left\{ z \cdot \frac{d^2z}{dt^2} + \left(\frac{dz}{dt}\right)^2 \right\};$$

$$\therefore \ \frac{d^2\phi}{dt^2} = \frac{2}{kr} \cdot \left\{ z \cdot \frac{d^2z}{dt^2} + v^2 \right\}; \quad \cdot \quad (13)$$

and substituting this value of $\dfrac{d^2\phi}{dt^2}$ in (12),

$$R = \frac{2M\rho^2}{kr^2 \left\{ \dfrac{k \cdot \sin\delta}{\sqrt{4z^2 (\sin\delta)^2 + k^2}} - \dfrac{2\mu_1 z}{\sqrt{4z^2 + k^2}} \right\}} \cdot \left\{ z \frac{d^2z}{dt^2} + v^2 \right\},$$

or, for brevity,

$$= A \left\{ z \cdot \frac{d^2z}{dt^2} + v^2 \right\},$$

or, substituting the value of $\dfrac{d^2z}{dt^2}$ derived from (10),

$$= A \left\{ \frac{G \cdot z}{M} - \frac{Rz}{M} \left(\frac{2z \cdot \sin\delta}{\sqrt{4z^2 (\sin\delta)^2 + k^2}} + \frac{\mu_1 k}{\sqrt{4z^2 + k^2}} \right) + v^2 \right\};$$

and from this expression may be deduced

$$R = \frac{2\rho^2 \{ Gz + Mv^2 \}}{\dfrac{(k^2r^2 + 4\rho^2 z^2) \sin\delta}{\sqrt{4z^2(\sin\delta)^2 + k^2}} + \dfrac{2\mu_1 kz (\rho^2 - r^2)}{\sqrt{4z^2 + k^2}}} \quad \cdot \quad \cdot \quad (14)$$

18. Equation (14) gives the pressure acting between the studs or rib of the projectile and the driving-surface of the groove at any point of the bore, and for any inclination of the driving-surface; but, as before stated, in the Woolwich guns the normal to the driving-surface (that is, the line of action of R) may, without material error, be considered as perpendicular to the radius.

If in (14) δ be put $= 90°$, the equation is simplified; and the resulting expression gives the total pressure on the studs for the Woolwich guns.

Putting then $\delta = 90°$, (14) becomes

$$R = \frac{2\rho^2 \sqrt{4z^2 + k^2}\,(Gz + Mv^2)}{kr^2(k - 2\mu_1 z) + 2\rho^2 z(2z + \mu_1 k)}. \quad \cdot \quad \cdot \quad (15)$$

19. Compare now (14) and (15), the equations giving the

pressure on the studs for parabolic rifling, with the equations subsisting where a uniform twist is used.

For a uniform twist we have, as I formerly showed,

$$R = \frac{2\pi\rho^2}{\dfrac{\mu_1(2\pi\rho^2 k - rh)}{\sqrt{1+k^2}} + \dfrac{(2\pi\rho^2 + rhk)\sin\delta}{\sqrt{k^2 + (\sin\delta)^2}}} \cdot G, \quad (16)$$

where h is the pitch of the rifling, k the tangent of the angle which the groove makes with the plane of xy, the other constants bearing the meaning I have already assigned to them in this investigation.

20. In the Woolwich guns, where $\delta = 90°$, (16) becomes

$$R = \frac{2\pi\rho^2\sqrt{1+k^2}}{hr(k-\mu_1) + 2\pi\rho^2(\mu_1 k + 1)} \cdot G, \quad (17)$$

21. I proceed to apply these formulæ, and propose to examine what are the pressures actually required to give rotation to a 400-lb. projectile, fired from a 10-inch gun with battering charges, under the following conditions :—1st. If the gun be rifled with an increasing twist as at present. 2nd. If it be rifled with a uniform pitch, the projectile in both cases being supposed to have the same angular velocity on quitting the gun. As the calculations for the uniform pitch are the simpler, I shall take this case first.

22. I have before remarked that with a uniform twist the pressure on the studs of the projectile is a constant fraction of that on the base of the shot, and represents, so to speak, on a reduced scale, the pressure existing at any point in the bore of the gun. Calling the fraction in equation (17) C, we have

$$R = C \cdot G, \quad (18)$$

where

$$C = \frac{2\pi\rho^2\sqrt{1+k^2}}{hr(k-\mu_1) + 2\pi\rho^2(\mu_1 k + 1)} = \cdot 04426, \quad (19)$$

the values of the constants in (19) being in the case of the 10-inch gun as follow :—

$\rho = \cdot 312$ ft., $k = 12\cdot 732$, $h = 33\cdot 333$ ft., $r = \cdot 417$ ft., $\mu_1 = \cdot 167$. Hence

$$R = \cdot 04426 \cdot G. \quad (20)$$

23. But the values of G are known with very considerable exactness from the investigations of the Explosive Committee under the presidency of Colonel Younghusband. The following Table gives the value of (G that is, the total pressure in tons acting on the base of the projectile) for a charge of 85 lbs. of

pebble-powder at various points of the bore, and the corresponding values of R. It will be remarked how high the pressure on the studs is when that on the base of the shot is a maximum, and how rapidly the strain decreases as the shot approaches the muzzle.

Table showing the pressure on the studs in a 10-inch British-service gun rifled with a uniform twist, calculated from (17).

Travel of shot, in feet.	Total pressure G on base of shot, in tons.	Value of C.	Value of R, or total pressure on studs, in tons.
0·000	0	·04426	0
0·333	1547	,,	68·5
0·945	1077	,,	47·7
1·834	781	,,	34·6
2·723	621	,,	27·5
3·612	510	,,	22·6
4·500	424	,,	18·7
5·389	356	,,	15·8
6·278	305	,,	13·5
7·167	268	,,	11·8
8·055	240	,,	10·6
8·944	220	,,	9·7
9·833	205	,,	9·1

24. The results in the Table show the pressures required to give rotation, if the 10-inch gun be rifled on a uniform twist. I turn now to the rifling as it actually exists, and which is defined to be a parabolic twist, commencing with one turn in 100 calibres and terminating at the distance of 9·833 feet with a twist of one turn in 40 calibres; and first to determine the equation to the parabola.

Let the origin be at the point where the twist vanishes when the curve A B is sufficiently prolonged—that is, at the vertex of the parabola. Let $O z$ and $O y'$ be the axes of coordinates; let

Fig. 2.
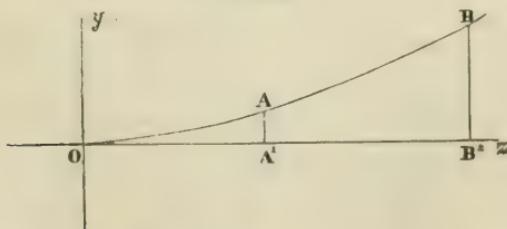
$O A' = z_1$, $O B' = z_2$; let $\tan \theta_1$ be the tangent of the angle which the curve makes with $O z$ at A, and $\tan \theta_2$ be the corresponding tangent at B.

Then, from the definition of the parabolic twist,

$$\frac{d^2y'}{dz^2} = \text{constant} = c, \text{ suppose};$$

$$\therefore \frac{dy'}{dz} = cz; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (21)$$

and

$$y' = \frac{c}{2}z^2. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (22)$$

But, from (21),

$$\tan \theta_2 = cz_2, \text{ and } \tan \theta_1 = cz_1;$$

$$\therefore c = \frac{\tan \theta_2 - \tan \theta_1}{z_2 - z_1} = \cdot 047925.$$

Comparing (22) with the form of this equation given in (4), $z^2 = kr\phi$, we have $y' = r\phi$ and $k = \frac{2}{c} = 417\cdot3$.

Hence the equation to the development of the parabolic rifling is

$$z^2 = 417\cdot3 r\phi, \quad . \quad . \quad . \quad . \quad . \quad (23)$$

and z_1 the distance of the origin from the commencement of the rifling $= \frac{\tan \theta_1}{c} = 6\cdot555$ feet.

25. As in the last case, I place in the form of a Table the results given by (15) for different values of z. The values of the constants are, $r = \cdot417$ feet, $k = 417\cdot3$, $\rho = \cdot312$ feet, $\mu_1 = \cdot167$, $M = \cdot00555$.

Table showing the pressure on the studs in a 10-inch British-service gun rifled with a parabolic twist commencing at one turn in 100 calibres and terminating at one turn in 40 calibres, calculated from (15).

Value of z, the distance from the origin, in feet.	Corresponding travel of the shot in the bore, in feet.	Corresponding velocity of shot, in feet.	Total pressure on base of shot, in tons.	Value of R, or total pressure on studs, in tons.
6·555	0·000	0	0	0
6·888	0·333	411	1547	31·2
7·500	0·945	675	1077	28·7
8·389	1·834	873	781	29·0
9·278	2·723	992	621	30·2
10·167	3·612	1078	510	31·4
11·055	4·500	1146	424	32·3
11·944	5·389	1200	356	33·0
12·833	6·278	1245	305	33·8
13·722	7·167	1282	268	34·5
14·610	8·055	1311	240	35·2
15·499	8·944	1333	220	35·8
16·388	9·833	1349	205	36·3

26. From an examination of the values of R given in this Table, it will be seen that the total pressure on the driving-surface reaches about 31 tons shortly after the commencement of motion, and the projectile quits the bore with a pressure of about 36 tons. With the view of making the variations which the pressures undergo more readily comparable, I have laid down in Plate VI. the curves derived from equations (15) and (17) for the battering charge of pebble-powder.

From these diagrams the pressures on the driving-surface at any point of the bore, both for the uniform and parabolic twists, can be seen by simple inspection. The line of abscissæ gives the travel of the shot, and the ordinates give the corresponding *total pressure* on the studs.

The curves show that with the uniform spiral the pressure on the studs reaches nearly 70 tons after a travel of ·3 feet, rapidly falling to about 9 tons at the muzzle, while with the parabolic rifling the pressure at ·3 feet of travel, corresponding to the point of maximum pressure, is only 31 tons. The pressure then falls slightly and amounts to 28 tons at about 1 foot travel; thence it gradually increases to a maximum of 36 tons at the muzzle.

By way of comparison, I have added in the Plate a curve showing the pressures required to give rotation to a 400-lb. projectile fired from the 10-inch gun with uniform twist when R. L. G. instead of pebble-powder is used.

The curve in this case is of the same nature as that derived from the pebble-powder; but the variation is greater, the maximum pressure being much higher and the muzzle-pressure, owing to the smaller charge, somewhat less.

27. To one more point it is worth while to call attention.

If the gun were a smooth-bore gun, the equation of motion would be

$$\text{M} \cdot \frac{d^2z}{dt^2} = \text{G}'; \quad \ldots \quad \ldots \quad (24)$$

and comparing this equation with (10), we have, on the supposition * that the velocity increments in both cases are equal,

$$\text{G} = \text{G}' + \text{R} \cdot \left\{ \frac{2z \cdot \sin \delta}{\sqrt{4z^2 (\sin \delta)^2 + k^2}} + \frac{\mu_1 k}{\sqrt{4z^2 + k^2}} \right\}, \quad (25)$$

or, in the case of the Woolwich gun, where $\delta = 90°$,

$$\text{G} = \text{G}' + \text{R} \cdot \left\{ \frac{2z + \mu_1 k}{\sqrt{4z^2 + k^2}} \right\}; \quad \ldots \quad \ldots \quad (26)$$

---

* Were the velocity increments not supposed equal, the reduction of pressure due to the suppression of rifling would be less than that given in the text.

and the interpretation of these equations is that the gaseous pressure in the rifled guns (rifled with the parabolic twist) is greater than that in the smooth-bored gun by the second term of the right-hand member of the equation.

28. The corresponding equations for a uniform twist are

$$G = G' + R \left\{ \frac{\sin \delta}{\sqrt{k^2 + (\sin \delta)^2}} + \frac{\mu_1 k}{\sqrt{1 + k^2}} \right\}, \quad . \quad (27)$$

or, if $\delta = 90°$,

$$G = G' + R \left\{ \frac{\mu_1 k + 1}{\sqrt{1 + k^2}} \right\}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (28)$$

29. I shall now put these results in actual figures, and shall again take for illustration the 10-inch gun, supposed (as before) to be rifled, 1st, on the uniform, 2nd, on the parabolic or service twist.

With the uniform twist, G (see Table) $= 1547$ tons; and using equation (28) and the values of the constants given in 22,

$$G' = G - \cdot 245 R$$

$$= \cdot 989 G. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (29)$$

Hence the decrement of pressure due to the suppression of rifling is only about 1 per cent.; that is, the total pressure on the base of the shot is reduced from 1547 tons to 1530 tons, or the bursting pressure is reduced from 19·7 tons per square inch to 19·5 tons per square inch.

At the muzzle of the gun in the same manner we find that the total pressure is reduced from 205 tons to 202·8 tons, and the pressure per inch in a corresponding proportion.

30. Similarly, from equation (26) and the values of the constants given in 25, the values of G′ at the point of maximum pressure and at the muzzle of the gun are obtained; and I find that with the parabolic twist the pressure on the base of the shot would be reduced from 1547 tons to 1541 tons, or the bursting pressure would be reduced from 19·7 tons to 19·62 tons per square inch.

At the muzzle the corresponding reductions are from 205 tons total pressure, to 196 tons, or from 2·61 tons to 2·49 tons per square inch.

31. For the sake of clearness, I recapitulate the results at which I have arrived. They are as follow :—

1st. That the pressures actually exerted at all points of the bore to give rotation to the 10-inch British-service projectile, compared with the pressures which would be exerted were the gun rifled on a uniform twist, are very approximately exhibited in the diagrams on Plate VI.

2nd. That in the 10-inch gun (and other guns similarly rifled)

the pressure on the studs due to rifling is but a small fraction (about 2¼ per cent.) of the pressure required to give translation to the shot.

3rd. That the substitution of the parabolic for the uniform rifling has reduced by about one half the maximum pressure on the studs.

4th. That the increment of the gaseous pressure, or the pressure tending to burst the gun, due to rifling is exceedingly small*, both in the case of the uniform and parabolic rifling. This result is entirely confirmed by the experiments of the Explosive Committee, who have found no sensible difference of pressure in the 10-inch gun fired in the rifled and unrifled states.

5th. That, small as the increment in gaseous pressure due to rifling is, it is still less in the parabolic than in the uniform system of rifling.

Elswick Works, February 15, 1873.

XXVII. *Note on the Measure of Intensity in the Theories of Light and Sound. By* R. H. M. Bosanquet, *Fellow of St. John's College, Oxford*†.

MR. MOON, and more recently Dr. Hudson, have, in the Philosophical Magazine, controverted the position that the energy of the forms of motion, which constitute light and sound, with factors depending on the wave-length or periodic time, is to be regarded as the measure of the intensity of these impressions on our senses. The matter is one for experimental evidence. Now direct evidence bearing on the point at issue is not wanting, in the case of either sound or light.

The question is whether the intensity is measured by the square of the amplitude, or by the amplitude, for given periodic times.

Mr. Moon has not offered any answer to the remark made at the end of my paper of last November, although, if he understood it, it is conclusive in the case of light. It may be worth while to mention the point again. There exists an experimental law known as the law of Malus, which connects the intensities of the two rays, polarized in planes at right angles to each other, into which a plane-polarized ray is decomposed by a doubly refracting crystal. The intensities of these two rays are proportional to the squares of the sine and cosine of the angle

---

* Although the increase of strain due to rifling is inconsiderable, yet the decrease of the strength of the structure of a gun inseparable from rifling may be, and in many systems is, considerable; but the discussion of this question is outside of the scope of my paper.

† Communicated by the Author.

which the principal plane of the crystal makes with the original plane of polarization. The verification is stated to have been made photometrically by Arago; but it is easy to obtain a verification for one's self. If the ray that has passed through a Nicol fall through a hole on a crystal of Iceland spar of suitable thickness so that the emerging rays overlap, it is quite easy to recognize that, as the crystal is turned round, the intensity of the overlapping part is constant. Now the amplitudes of the polarized pencils will be

$$a \cos \alpha \text{ and } a \sin \alpha,$$

if a be the amplitude of the original ray, and α the inclination of the principal plane of the prism to the plane of polarization of the Nicol. But if these amplitudes were the measures of the intensities, the intensity of the overlapping part would be represented by $a (\sin \alpha + \cos \alpha)$, and would vary with α. The question is here submitted to direct experiment; and we see that it is only by taking the intensity to be measured by the square of the amplitude that the experimental result can be accounted for; we have then, of course, $a^2 (\sin^2 \alpha + \cos^2 \alpha) = a^2$.

In the case of sound, the law of Töpfer, which is quoted in my paper in the Philosophical Magazine, November 1872, shows that in organ-pipes of equal intensity the wind consumed is proportional to the wave-length. As I have remarked, this is equivalent to saying that the work done is proportional to the wavelength. Töpfer's law is beyond dispute. It might well have been left to rest on his authority, except that, for once, he gives no measures or evidence for it; but he is generally so accurate that his enunciation of the law carries weight. The evidence in the case of sound does not at present amount to actual proof of the representation of intensity by mechanical energy, but to a high degree of probability. I have devised a better method of observation, which requires special arrangements, and hope to throw some additional light on the subject when I have an opportunity of carrying this out.

As one of Mr. Moon's suggestions is sometimes felt as a difficulty by learners, I will just touch upon it. It seems specious to say that, if we superpose two luminous or sonorous vibrations of the type $y = a \sin \dfrac{2\pi}{\lambda} (vt - x)$, the amplitude of the resulting vibration will be $2a$, and the intensity four times that of either vibration. But if we draw from this the conclusion that from twice the energy proportional to a^2 we obtain energy proportional to $4a^2$, we clearly make a mistake. In the first place, if the two vibrations are to be superposed they must be in the same phase; this requirement prevents the occurrence of the super-

position in nature in many cases. Thus, if two similar organ-pipes are sounded near each other in exact unison, they always arrange their vibrations in opposite phases, and the two tones destroy each other. No doubt also the explanation given by Airy of the doubling of intensity by use of two candles points in the correct direction. As, however, it takes no account of the alteration of the plane of polarization in common light, a matter of which we know scarcely any thing, it can only be regarded as an illustration. The complete explanation cannot be given or recognized till we know more about the alteration of the plane of polarization. We must note, however, that the doubled light of two candles is not to be regarded as an axiom, but as a fact ascertained in the every-day processes of photometry.

Still it may often happen that two such vibrations may be superposed with coincident phase; it happens, for instance, at the loudest part of the beats given by two organ-pipes nearly in unison. In these cases we must not treat each vibration as a cause in itself, invariable under all conditions. If we regard the two vibrations as unaltered by the superposition, we shall in general be wrong. We must go back to the sources of energy; and we shall in general find that the delivery of the vibrations is materially affected by their superposition. By the superposition of the vibrations we increase the work to be done by each, if considered to remain unaltered. Take an illustration from electricity; then it is like interposing an additional resistance in an electric circuit, only that heat is developed instead of sound. If the battery be arranged for quantity, as it is called, the current may be materially affected by the new resistance; if for tension, the new resistance may have a small influence only. So with an organ-pipe. Let two pipes be nearly in unison, and at the swell of the beat, when the vibrations are superposed with the same phase; then the state of things may vary a good deal, but necessarily lies between the following extreme cases.

First, let the wind be very light, passing with freedom through the windway, and wasting little of its pressure there on the overcoming of friction, imparting velocity to the external air, and so on; then most of the pressure at the windway is due to the work converted into sound. The pressure at the orifice, and thence the rate of issue, are then different as more or less work is converted into sound; when the work to be done is increased, the pressure at the windway is increased and the velocity lessened. Thus on superposition the amplitude of each vibration will be diminished, until the work of the compound vibration is such as the sources are able to furnish; and in the extreme case each amplitude would be diminished in the ratio of $1 : \sqrt{2}$. This is analogous to the introduction of a resistance into an electrical

circuit of small resistance, or where the battery is arranged for quantity.

Secondly, if the organ have a heavy wind, and the pipes have narrow windways, so that the greater portion of the pressure at the windway is due to friction, velocity communicated to the external air, &c., and only a small part to the reaction of the sound-impulses, then even considerable variations of the work transformed into sound will not alter perceptibly the pressure at the windway or the velocity of exit : thus, on superposition, the two vibrations would retain their form, and the intensity of the swell of the beat would be in the extreme case four times that of the single vibration. This case is analogous to the introduction of a resistance into an electric circuit whose resistance is already great, or where the battery is arranged for tension.

XXVIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

December 5, 1872.—Rear-Admiral G. H. Richards, C.B., Vice-President, in the Chair.

THE following communication was read :—
" Investigation of the Attraction of a Galvanic Coil on a small Magnetic Mass." By James Stuart, M.A., Fellow of Trinity College, Cambridge.

From investigations given by Ampère, we can deduce an expression for the potential U at an external point Q of a closed circular galvanic current carried by a wire of indefinitely small section. Let a be the radius of the circle; let the distance of Q from C, the centre of the circle, be r ; and let the line C Q make an angle θ with the normal to the plane of the circle. Then it can be shown that when r is less than a,

$$U = 2\pi k \left\{ -1 + \frac{r}{a} P_1 - \frac{1}{2} \frac{r^3}{a^3} P_3 + \frac{1 \cdot 3}{2 \cdot 4} \cdot \frac{r^5}{a^5} \cdot P_5 - \ldots \right\} ;$$

and when r is greater than a,

$$U = 2\pi k \left\{ -\frac{1}{2} \frac{a^2}{r^2} P_1 + \frac{1 \cdot 3}{2 \cdot 4} \cdot \frac{a^4}{r^4} P_3 - \frac{1 \cdot 3 \cdot 5}{2 \cdot 4 \cdot 6} \cdot \frac{a^6}{r^6} P_5 + \ldots \right\} ,$$

where k depends only on the intensity of the current, and where P_1, P_3, P_5 are defined by the equation

$$\frac{1}{\sqrt{1 - 2x \cos \theta + x^2}} = 1 + P_1 x + P_2 x^2 + P_3 x^3 + \ldots .$$

If, therefore, X represents the resolved part perpendicular to the plane of the circle and towards it of the force exerted by the current on a unit of magnetism placed at Q, and if Y represent the resolved part of that force parallel to the plane of the circle and directed

from its centre outwards, then

$$X = \frac{dU}{r \cdot d\theta} \sin \theta - \frac{dU}{dr} \cos \theta,$$

$$Y = \frac{dU}{r \cdot d\theta} \cos \theta + \frac{dU}{dr} \sin \theta.$$

To calculate these quantities, we know that

$$P_1 = \cos \theta,$$
$$P_2 = \frac{5}{2} \left(\cos^3 \theta - \frac{3}{5} \cos \theta \right),$$
$$P_3 = \frac{63}{8} \left(\cos^5 \theta - \frac{10}{9} \cos^3 \theta + \frac{15}{63} \cos \theta \right).$$

We shall only consider the case of those points for which r is greater than a. Substituting these values in the expression which in such instances holds for U, we have

$$U = 2\pi k \left\{ -\frac{1}{2} \cdot \frac{a^2}{r^2} \cos \theta + \frac{15}{16} \cdot \frac{a^4}{r^4} \left(\cos^3 \theta - \frac{3}{5} \cos \theta \right) \right.$$
$$\left. - \frac{315}{128} \cdot \frac{a^6}{r^6} \left(\cos^5 \theta - \frac{10}{9} \cos^3 \theta + \frac{15}{63} \cos \theta \right) \right.$$
$$\left. + \ldots \right\}.$$

From which, after some reduction, we obtain

$$\frac{X}{2\pi k} = -\frac{1}{2} (-1 + 3 \cos^2 \theta) \cdot \frac{a^2}{r^3} + \frac{1}{16} \cdot (9 - 90 \cos^2 \theta + 105 \cos^4 \theta) \frac{a^4}{r^5}$$
$$- \frac{1}{128} (-75 + 1575 \cos^2 \theta - 4725 \cos^4 \theta + 3465 \cos^6 \theta) \frac{a^6}{r^7}$$
$$+ \ldots \ldots, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

$$\frac{Y}{2\pi k} = \sin \theta \cdot \left\{ + \frac{3}{2} \cos \theta \cdot \frac{a^2}{r^3} - \frac{1}{16} (-27 \cos \theta + 105 \cos^3 \theta) \frac{a^4}{r^5} \right.$$
$$\left. + \frac{1}{128} (525 \cos \theta - 3150 \cos^3 \theta + 3465 \cos^5 \theta) \frac{a^6}{r^7} \right.$$
$$\left. - \ldots \ldots \right\} . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Each of these expressions consists of a series of terms in ascending powers of $\frac{a}{r}$ which will be converging.

We shall now seek to find X and Y for a galvanic current traversing a wire coiled into the form of a hollow cylinder, of which the internal radius is b, the external radius $b + c$, and of which the length is $2f$. We shall suppose the individual turns of the wire to lie so close that each may be regarded as an exact circle.

Let A B be the axis of the coil, so that A and B are the centres of its two faces; then A B $= 2f$. Let O be the middle point of A B.

Let P be the attracted point, P M its perpendicular distance p from A B. Let P A M$=\alpha$, P B M$=\beta$.
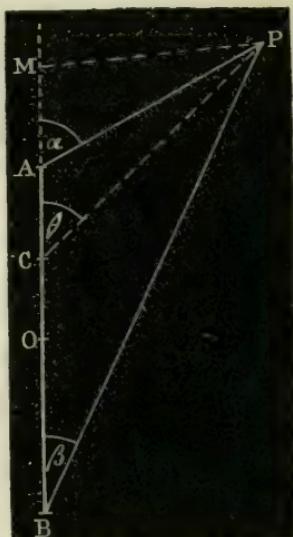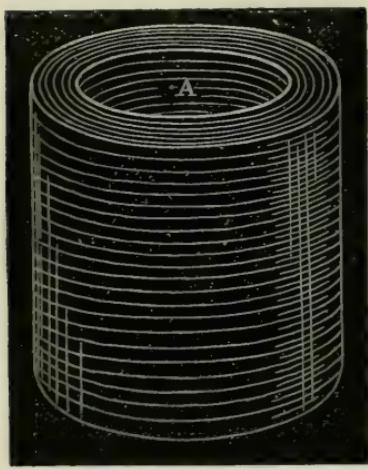
Let C be the centre of any turn of the wire regarded as a circle of radius a, C P$=r$, P C M$=\theta$, O C$=x$; then it is readily seen that for the whole cylindrical bobbin the forces X, Y are given by

$$\frac{X}{\mu} = \int_{-f}^{+f}\int_{b}^{b+c} L\,dx\,da,$$

$$\frac{Y}{\mu} = \int_{-f}^{+f}\int_{b}^{b+c} M\,dx\,da,$$

where L and M stand for the expressions on the right-hand side of (1) and (2) respectively, and where μ depends on the strength of the current.

To perform the integrations for the length of the bobbin in these expressions, we have the formulæ

$$p = r \cdot \sin\theta,$$
$$\delta x \cdot \sin\theta = -r \cdot \delta\theta;$$
$$\therefore \delta x = \frac{-p\,\delta\theta}{\sin^2\theta},$$

and

$$r = \frac{p}{\sin\theta}.$$

Making these substitutions for δx and r, the integrals with respect to x become integrals with respect to θ, which can be easily evaluated by a continued application of the method of integration by parts, the limits being from $\theta = \alpha$ to $\theta = \beta$. If we then integrate the result thus obtained with respect to a, from the limit b to the limit $b+c$, we finally obtain

$$\frac{X}{\mu} = \frac{\overline{b+c}^3 - b^3}{6\,p^2}\left\{-(\cos\beta - \cos\alpha) + (\cos^3\beta - \cos^3\alpha)\right\}$$

$$+\frac{\overline{b+c}^5 - b^5}{80\,p^4}\left\{-9(\cos\beta - \cos\alpha) + 33(\cos^3\beta - \cos^3\alpha)\right.$$

$$\left. -39(\cos^5\beta - \cos^5\alpha) + 15(\cos^7\beta - \cos^7\alpha)\right\}$$

$$+\frac{\overline{b+c}^7 - b^7}{896\,p^6}\left\{-\;75(\cos\beta - \cos\alpha)\;+\;575(\cos^3\beta - \cos^3\alpha)\right.$$

$$-1590(\cos^5\beta - \cos^5\alpha) + 2070(\cos^7\beta - \cos^7\alpha)$$

$$+\ldots\ldots,\qquad -1295(\cos^9\beta - \cos^9\alpha) + \;315(\cos^{11}\beta - \cos^{11}\alpha)\big\}$$

$$\frac{Y}{\mu} = \frac{\overline{b+c}^3 - b^3}{6p^2}\left\{+(\sin^3\beta - \sin^3\alpha)\right\}$$

$$+\frac{\overline{b+c}^5 - b}{80p^4}\left\{-12(\sin^5\beta - \sin^5\alpha) + 15(\sin^7\beta - \sin^7\alpha)\right\}$$

$$+\frac{\overline{b+c}^7 - b^7}{896p^6}\left\{+120(\sin^7\beta - \sin^7\alpha) - 420(\sin^9\beta - \sin^9\alpha)\right.$$

$$+\ldots\ldots\quad +315(\sin^{11}\beta - \sin^{11}\alpha)\big\}$$

These expressions for X and Y will be converging for all points situated at a greater distance than $b+c$ from any point of the axis A B, inasmuch as they are composed by adding together corresponding terms of series which are then all convergent. Among other points, these expressions hold for such as are situated on the axis external to the bobbin, and not nearer A or B than by the distance $(b+c)$. For such points, however, the expressions become illusory, assuming the form $\frac{0}{0}$. They may, however, be evaluated by the methods for the evaluation of vanishing fractions. Y is clearly zero. X may be more readily obtained directly from the expression for U. From that expression we find that for a single circular current the attraction on such points is

$$X = 2\pi k\left\{+\frac{a^2}{r^3} - \frac{3}{2}\frac{a^4}{r^5} + \frac{15}{8}\frac{a^6}{r^7} - \ldots\right\}.$$

Hence, in the case of a bobbin, if x be the distance of the attracted point from O, the middle point of the axis of the bobbin, we have

$$\frac{X}{\mu} = \int_{x+f}^{x-f}\int_{b}^{b+c} dr\,da\left(+\frac{a^2}{r^3} - \frac{3}{2}\frac{a^4}{r^5} + \frac{15}{8}\frac{a^6}{r^7} - \ldots\right)$$

$$= -\frac{\overline{b+c}^3 - b^3}{6(x^2-f^2)^2}(\overline{x+f}^2 - \overline{x-f}^2)$$

$$+3\frac{\overline{b+c}^5 - b^5}{40(x^2-f^2)^4}(\overline{x+f}^4 - \overline{x-f}^4)$$

$$-5\frac{\overline{b+c}^7 - b^7}{112(x^2-f^2)^6}(\overline{x+f}^6 - \overline{x-f}^6)$$

$$+\ldots\ldots,$$

which gives X for points situated on the axis for which x is not less than $(b+c+f)$.

January 9, 1873.—William Sharpey, M.D., Vice-President, in the Chair.

The following communication was read :—

"On a new Method of viewing the Chromosphere." By J. N. Lockyer, F.R.S., and G. M. Seabroke.

The observations made by slitless spectroscopes during the eclipse of Dec. 11, 1871, led one of us early this year to the conclusion that the most convenient and labour-saving contrivance for the daily observation of the chromosphere would be to photograph daily the image of a ring-slit, which should be coincident with an image of the chromosphere itself.

The same idea has since occurred to the other.

We therefore beg leave to send in a joint communication to the Royal Society on the subject, showing the manner in which this kind of observation can be carried out, remarking that, although the method still requires some instrumental details, which will make its working more perfect, images of the chromosphere, almost in its entirety, have already been seen on several days during the present month and the latter part of last month.

The adaptation of this method to a telespectroscope will be seen at a glance from the accompanying drawing.
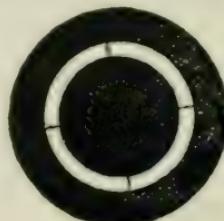
Diaphragm showing annulus, the breadth of which may be varied to suit the state of the air.

The annulus is viewed and brought to focus by looking through apertures in the side of the tubes.

A. Sliding eye-tube of telescope.   B. Tube screwing into eye-tube.   C. Tube sliding inside B, and carrying lens D and diaphragm E.   F. Lenses bringing image of diaphragm to a focus at the place generally occupied by the slit of the spectroscope.   G. Collimator of spectroscope.

The image of the sun is brought to focus on a diaphragm having a circular disk of brass (in the centre) of the same size as the sun's image, so that the sun's light is obstructed and the chromospheric light is allowed to pass. The chromosphere is afterwards brought to a focus again at the position usually occupied by the slit of the spectroscope; and in the eyepiece is seen the chromosphere in circles corresponding to the " C " or other lines. The lens D is used to reduce the size of the sun's image, and keep it of the same size as the diaphragm at different times of the year; and the lenses F are used in order to reduce the size of the annulus of light to about $\frac{1}{8}$ inch, so that the pencils of light from either side of the annulus may not be too divergent to pass through the prisms at the same time, and that the image of the whole annulus may be seen at once. There are mechanical difficulties in producing a perfect annulus of the required size, so one $\frac{1}{2}$ inch in diameter is used, and can be reduced virtually to any size at pleasure.

The proposed photographic arrangements are as follows :—

A large Steinheil spectroscope is used, its usual slit being replaced by the ring one.

A solar beam is thrown along the axis of the collimator by a heliostat, and the sun's image is brought to focus on the ring-slit by a $3\frac{3}{4}$-inch object-glass, the solar image being made to fit the slit by a suitable lens.

By this method the image of the chromosphere received on the photographic plate can be obtained of a convenient size, as a telescope of any dimensions may be used for focusing the parallel beam which passes through the prisms on to the plate.

The size of the image of the chromosphere obtained by the method adopted will be seen from the accompanying photograph, taken when the ring-slit was illuminated with the vapours of copper and cadmium.

December 6, 1872, at 11.30.   December 7, 1872, at 11.30.
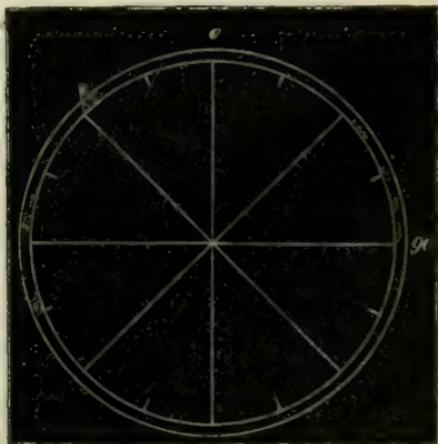
Outer circle 100″ from inner one. Chromosphere at normal height. except where prominences marked.

As this photograph is not reproduced, it may be stated that the ring-images have an internal diameter of nearly $\frac{3}{4}$ of an inch.

The accompanying solar profiles are copies of drawings made, on the dates stated, by means of the new method, which were exhibited by the authors at the Meeting.

[Since reading the above paper, it has come to our knowledge that Zöllner had conceived the same idea unknown to us, but had rejected it. Prof. Wenlock in America has tried a similar arrangement, but without success.—J. N. L., G. M. S., January 17, 1873.]

January 16.—T. Archer Hirst, Ph.D., Vice-President, in the Chair.

The following communication was read :—

"A new Formula for a Microscope Object-glass." By F. H. Wenham.

A pencil of rays exceeding an angle of 40° from a luminous point cannot be secured with less than three superposed lenses of increasing focus and diameter, by the use of which combination rays beyond this angle are transmitted, with successive refractions in their course, towards the posterior conjugate focus : until quite recently, each of these separate lenses has been partly achromatized by its own concave lens of flint glass, the surfaces in contact with the crown glass being of the same radius, united with Canada balsam ; the front lens has been made a triple, the middle a double, and the back again a triple achromatic. This combination therefore consists of eight lenses, and the rays in their passage are subject to errors arising from sixteen surfaces of glass.

In the new form there are but ten surfaces, and only *one* concave lens of dense *flint* is employed for correcting *four* convex lenses of *crown* glass : as this might at first sight be considered inconsistent with theory, a brief retrospect of the early improvements of the microscope object-glass will help to define the conditions. The knowledge of its construction has been entirely in the hands of working opticians ; and the information published on the subject being scanty, this has probably prevented the scientific analyst from giving that aid which might have been expected.

Previous to the year 1829 a few microscopic object-glasses were made, composed of three superposed achromatic lenses ; but this combination appears to have been used merely with the intention of gaining an increase of power, in ignorance of any principle, and without even a knowledge of the value of angular aperture.

At this time the late J. J. Lister tried a number of experiments, and discovered the law of the aplanatic focus, and proved that, by separating lenses suitably corrected, there were one or two positions in which the spherical aberration was balanced. This was explained in a paper read before the Royal Society in 1829. In the year 1831 Mr. Ross was employed to construct the first achro-

matic object-glass in accordance with this principle, which performed with "a degree of success never anticipated."

Mr. Ross then discovered that, after he had adjusted the interval of his lenses for the aplanatic focus, that position would no longer be correct if a plate of thin glass was placed above the object; this focus had then to be sought in a different plane, and the lenses brought closer together, in order to neutralize the negative aberration caused by covering-glass of various thickness. From this period the "adjustment" with which all our best object-glasses are now provided became established. Fig. 1 is the form of object-glass used at this time, consisting of three plano-concave achromatics, whose foci were nearly in the proportion of 1, 2. 3.

No greater angle than 60° could be obtained with this system in a $\frac{1}{8}$-inch objective (the highest power then made), for reasons apparent in the diagram. The excessive depth of curvature of the contact-surfaces of the front pair is unfavourable for the passage of the marginal rays; the softness of the flint glass forming the first plane was also objectionable. In the year 1837 Mr. Lister gave Mr. Ross a diagram for an improved "eighth," having a *triple front lens* in the form shown in fig. 2. By this the passage of extreme rays was facilitated; and in order to diminish the depth of curvature, a very dense glass was used, having a specific gravity of 4·351. Faraday's glass, having a density of 6·4, had been previously tried, but was abandoned on account of a difficulty in working it. The polished surfaces of both these qualities of dense glass speedily became tarnished by exposure to the air; and thus the dense flint concave could only be employed in a triple combination, that is, when cemented between two lenses of crown glass: this form of front was kept a trade secret, and was not published in any work treating of the optics of the microscope. The front incident surface of the flint of the middle pair was made concave, in order to reduce the depth of the contact; and for this reason only, as that surface has but little influence in correcting the oblique pencils, or in producing flatness of field, and may be a plane with an equally good or better result. "Eighths" of this form with angles of 80° were made, and remained unaltered till the year 1850, when larger apertures were called for, and Mr. Lister introduced the *triple back lens.*

The necessity for this will be seen by the diagram (fig. 2) which shows that the contact-surfaces of the back achromatic are too deep, thus giving great thickness to the lens and limiting its diameter: dense flint would have remedied this to some extent; but its liability to tarnish rendered its use in a *pair* objectionable. The highest density at this time known, quite free from this defect, was 3·686. By means of the triple back, the final corrections were rendered less abrupt, a greater portion of the marginal rays could be collected, and the aperture of an "eighth" was at once brought up to 130° or more.

At this time the author had been making some experiments

in the construction of an object-glass in the form of fig. 2. Mr.
Lister having favoured his " eighth" with an examination, was
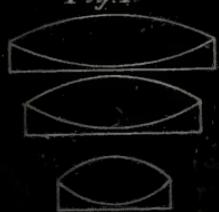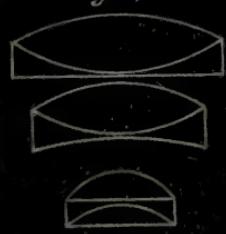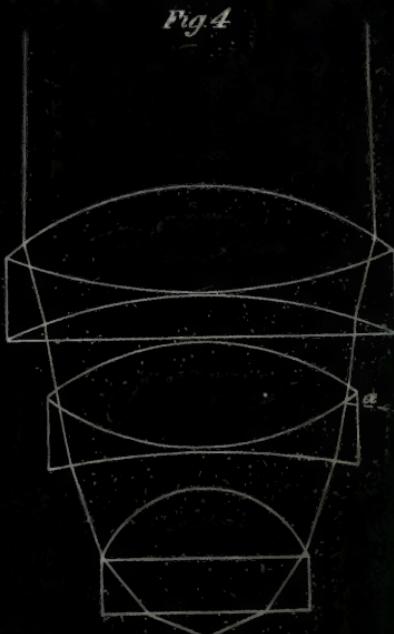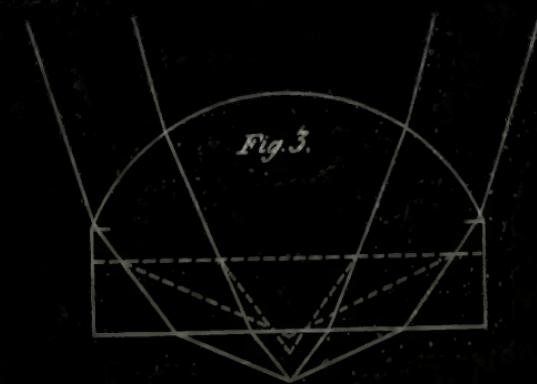
good enough to communicate his late improvement of the triple back. No time was lost in giving this a trial, the result of which proved that excessive negative aberration or over-correction could readily be commanded with lenses of shallow contact-curves. During these trials all chromatic correction was obtained by alterations in the triple *back*; for it was found that the colour-correction could not be controlled by a change in the concave surface of the triple *front*, as the negative power of the flint here appeared to be feeble, requiring a great difference in radius to give a trifling result. For this reason the front concaves were formed of very dense and highly dispersive flint; the cause of this was analyzed by a large diagram, with the passage of the rays projected through the combination, starting from the longest conjugate focus at the back. This proved that the rays from that focus passed through the concave flint of the front nearly as a radius from its centre, or in such a direction that its negative influence was almost neutralized. It is well known that a lens may be achromatic for parallel rays, and under-corrected for divergent ones. The utmost extent of this condition was apparent in the object-glass under consideration.

This led the author to the idea of the *single front* lens of crown glass, which gave a fine result at the first attempt, as the back combinations to which it was applied happened to have a suitable excess of negative or over-correction existing in the triple back alone, the middle being neutral or nearly achromatic. Still there was a defect remaining as positive spherical aberration; and this was afterwards cured by giving *additional thickness* to the *front* lens, which is now recognized as a most essential element of correction. In a " fifteenth," for instance, a difference of thickness of only ·002 of an inch will determine the quality between a good and an indifferent glass. Fig. 3 represents a front lens suitable for bringing the back rays to a focus. The dotted lines indicate the effect of this difference, showing that with a lens of less thickness the marginal rays fall within the central, producing positive aberration as the result.

The single front introduced by the author is now used by every maker; for several years he could not induce the leading opticians to change their system, though challenged by a series of high powers constructed on this formula, for the purpose of proving its superiority. Fig. 4 represents the curves of the first successful " eighth " on this system, having an aperture of 130°, enlarged ten times. On tracing the passage of the marginal rays through the combination, it will be seen that, though the successive refractions are nearly equalized, the contact-surfaces of the middle pair are somewhat deep, though no over-correction existed or was needed here, for this would have required a shorter radius still (the density of the flint in this was 3·686). If this pair of lenses were not cemented with Canada balsam, total reflection would take place near the circumference of the contact flint surface, cutting off the marginal rays at *a*, and limiting the aperture. It might be argued that

pra·tically this would be no disadvantage, as these surfaces are united with Canada balsam, whose refraction is higher than the crown; so that the rays in this case must proceed with very little deviation. But incidences beyond the angle of total reflection may be considered detrimental, as they imply excessive depth of curvature; this can be discovered by looking through the front of an object-glass held close to the eye, any air-films in the balsam near the edge of the lens appearing as opaque black spots.

At the commencement of the present year the author caused a few object-glasses to be made, with a middle of the form of fig. 5, the performance of which was very satisfactory. In this the extreme rays pass at more favourable incidences, and within the angle of total reflection. The upper lens is of dense flint.

When the experiments on the single front were concluded, and the remarkable corrective power of the triple back in conjunction therewith had been proved, the next attempt was to make the *middle* also a single lens, leaving the entire colour-correction to be performed by the one biconcave flint in the back. After numerous trials it was found that though something like over-correction or negative aberration could be obtained with the back, in the degree requisite for balancing the under-correction of the single middle and front when set at the prescribed distance of the aplanatic focus, yet by trial on the mercury globule all the results invariably displayed two separated colour-rings: these could not be combined by any alteration in the radius of the lenses. By projecting the blue and red, or visible rays of greatest and least refrangibility through the system, the cause became apparent. The left-hand section of this object-glass is shown in fig. 6. The rays from the focus are slightly divided by the first front surface. On emerging from the back the separation is increased; the red ray (r) is outwards, and the more refrangible or blue ray (b) inwards. Next, the divergence of these two rays is extended by the middle single lens. The following crown lens extends the angle of divergence so far that the flint lens of the back triple cannot recombine them; and they emerge at two distinct zones, shown by the practical test of the "artificial star" or light-spot reflected from a mercury globule, viewed within and without the focus.

It might be supposed that these rays at their final emergence can be so refracted as to project the blue outwards. A crossing point would then occur at a fixed conjugate focus in the body of the microscope, at which all rays would be combined; and if this focus was adjusted to that of the eyepiece, achromatism and final correction would be the result. But to meet the various conditions occurring in the use of the microscope, the conjugate focus constantly alters in position, this being affected by every change of eyepiece, length of tube, or adjustment for thickness of cover; therefore a correction for a fixed point cannot be maintained. Achromatism in the microscopic object-glass, like that of other perfectly corrected optical combinations, must be the reunion of the rays of the spectrum close to the final emergent surface of

the system. The remedy suggested by these experiments appeared to be in a transposition—that is, in placing the over-corrected triple

Fig. 6.
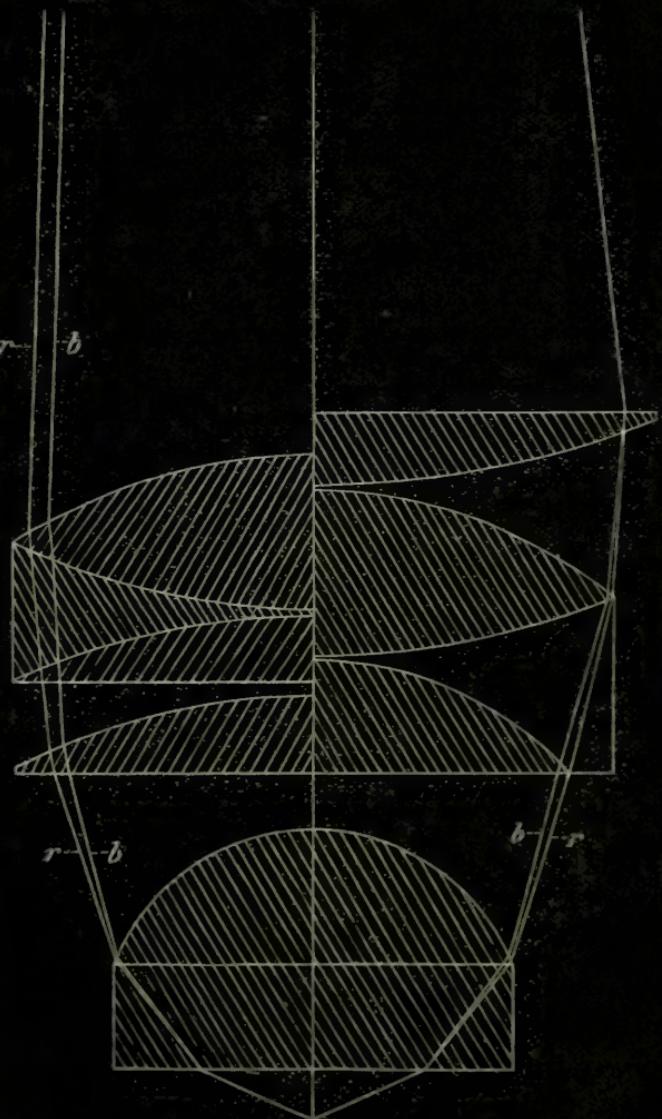
in the *middle* of the entire object-glass; this would at once cause a convergence of the blue and red rays. A single lens of longer focus at the back would then bring these rays parallel at the point of final emergence.

By projection in a diagram this condition was apparently realized. The dispersive power of the flint (density 3·686) was taken by the refractive index 1·76 of line H in the blue ray of the spectrum, and 1·70 of line B in the red ray. The refraction of the corresponding rays in the crown (density 2·44) was 1·53 H and 1·51·B. With these indices the rays are traced in fig. 6. The radii in the right-hand half section are those of an " eighth " of the new form drawn twenty times the size of the original. The *single front* is of the usual form, as this is much alike in all cases. The radius or focus of the *single plano-convex back* is about four and a half times that of the front, and the focus of the *middle* (triple) three times. The passage of the blue and red rays at the extreme of the pencil is shown in contrast with the preceding, the separation from the same front being alike.

The inner and outer, or blue and red rays, after passing the first surface of the triple middle, meet the concaves of the flint, which refract the blue rays to a greater extent than the red, and cause them to converge (instead of diverging, as in the opposing half diagram), so that at their exit from the triple they meet and would cross, effecting what is known as " over-correction;" but this is so balanced and readjusted by the single back of crown glass, that the rays are finally united, and emerge in a state of parallelism. This form of object-glass is suitable for the high powers, or such as have a cover adjustment, viz. from the "½-inch" upwards; perfect colour-correction is equally to be obtained in all of them.

It may be asked by some who have devoted their attention to the higher branches of optical mathematics, why the above result should have been worked out entirely by diagrams. But it has been found such a difficult task to calculate the passage of the two rays of greatest and least refrangibility through a combination having sixteen surfaces of glass of three different densities and refractions, that even first-class mathematicians have hitherto shrunk from the attempt.

Diagrams, however, are surprisingly accurate in their capability of indicating causes and results in the microscope and object-glass; for these lenses are minute, with deep curves and abrupt refractions; so that if the projection is worked out some fifty times the size of the original, small errors can be detected. The work should be commenced at the back from a long conjugate focus, which, not being a constant distance, may be taken as very near to parallelism. The high powers all have the means of correction within this distance, and perform better with a long posterior focus than with a very short one. The relative indices for the two or more rays should be marked on a large pair of proportional compasses, the long limb representing the sine of the angle of incidence, and the short one that of refraction. Both the sines ought to be

set off in the diagram *behind*, and neither of them in front of the ray in course of projection ; this leaves the way clear, with the least confusion of lines.

At the same time a second or counterpart diagram should be at hand, to which the rays only are transferred as soon as their direction is ascertained ; with these precautions a mistake is scarcely possible.

Now it is hoped that some improvements may be effected by this investigation, on account of the simplicity attained in the combination, in which we have *two single lenses* of crown, whose foci bear a definite proportion to each other ; while all the corrections are performed by *one concave of dense flint*, the acting condition of which is not altered by the influence of any other concaves acting in the combination, and hitherto taking a share of the duty. This one flint is now to be considered singly as the heart and centre of the system in reference to the correction of the rays entering and leaving.

This memoir is of necessity incomplete, for want of definite information concerning the optical properties of various kinds of glass. Data obtained from working them into small lenses furnish only a rough approximation to the mean dispersive power of the combined flint and crown having the best apparent effect. Of the intermediate rays, little can be known beyond the mere appearance of more or less of a secondary spectrum.

Nothing of importance has been published since Fraunhofer's Table, containing the refractive indices for each of the seven primary colour-lines of the spectrum for ten kinds of glass : great advance has been effected since that date in the manufacture of optical glass, a most complete collection of which of every variety has been made by the Rosses up to the present date. Selected specimens from this will be worked into prisms, and the relative spectra mapped out by the Fraunhofer lines, leading, it is hoped, to the discovery of a combination of crown and flint glass which shall be free from secondary spectrum or absolutely achromatic. The result of this investigation will be the subject of a future communication.

GEOLOGICAL SOCIETY.

[Continued from p. 152.]

June 19, 1872.—Prof. Ramsay, V.P.G.S., in the Chair.

The following communications were read :—

1. " On *Trochocyathus anglicus*, a new species of Madreporaria from the Red Crag." By P. Martin Duncan, M.B., F.R.S., V.P.G.S., Professor of Geology in King's College, London.

The author described a Coral of which a single specimen had been found in the Red Crag, in the grounds of Great Bealings Rectory, Norfolk. He stated that it belonged to the genus *Trochocyathus*,

and was distinguished from the other species of that genus by its dense epitheca, its small and prominent columella, and its inverted calicular margin. He proposed to name it *Trochocyathus anglicus*, and stated that its nearest alliance is with the Australian Upper Tertiary form described by him under the name of *T. meridionalis*.

2. " On the Discovery of Palæolithic Implements in association with *Elephas primigenius* in the High-terrace Gravels at Acton and Ealing." By Col. A. Lane Fox, F.G.S.

The gravels in the neighbourhood of Acton have been divided by Mr. Prestwich into two principal groups, viz. the high-level gravels, on the hills above the valley, and the valley-gravels, on the sides and bottom of the valley itself. The valley-gravels have been again divided by Mr. Whitaker into three terraces, viz. a high terrace, between 50 and 100 feet above the Ordnance datum, a mid terrace, between 20 and 40 feet high, and a low terrace, at an average height of 10 feet, occupying the low ground in the bends of the river. On both sides of the river the high terrace is separated from the mid terrace by a strip of the London Clay, which is laid bare at an average level of 50 feet. The London Clay is also laid bare on the sides of the tributary streams running into the valley on both sides of the river, thus dividing the high-terrace gravel into patches. The mid terrace is continuous, and follows the sinuosities of the valley on both sides up to the strip of London Clay. The author accounts for this distribution of the gravels by supposing that a large body of water must at one time have stood at the 50-feet level, and the denudation of the high terrace have been caused by the waves beating on the sides of the valley, and by drainage into this body of water. The mid terrace he conceives may have been caused in part by accumulations beneath this body of water.

The position of the high-terrace gravel at Acton corresponded so closely to that of the implement-bearing gravels of the Somme and the Ouse, that the author was led to examine carefully the excavations made in it for the construction of houses. He discovered a number of implements of the drift-type, together with flakes and cores, and a few roughly formed scrapers ; all these were found in close contact with the London Clay, and beneath the gravel. Fragments of fern (*Osmunda regalis*) and of wood (*Pinus sylvestris*) were also found with the implements at the same level. Two implements were found at Ealing Dean, 2 miles westward, on nearly the same level as those of Acton, viz. 90 feet ; and these also came from the bottom of the gravel. Another implement was found south of the river at Battersea Rise, in the same position above the strip of London Clay as at Acton, and at about 60 feet above the Ordnance datum. The implements are of the pointed and oval types. The only animal remains discovered in the high terrace consisted of a tooth of *Elephas primigenius* in the Acton gravel. The position of this the author believes to be reliable, although he did not discover it himself *in situ*.

In the mid-terrace gravel a number of pits were examined be-

tween Shepherd's Bush and Hammersmith, and in the neighbour-hood of Turnham Green, which resulted in the discovery, at the latter place, of a large quantity of animal remains (noticed by Mr. Busk in the following paper), all of which, like the implements of the high terrace, were at the bottom of the gravel; but no evidence of human workmanship was found in the mid terrace.

All these were found together, in the same seam of gravel, 12 feet beneath the surface; and all appeared to have been deposited at the same time. The surface was here 25 feet above the Ordnance datum, and consequently about 50 feet lower than the implements of the high terrace, $1\frac{1}{2}$ mile to the north. The section across the valley, taken through the two places, here shows the strip of the London Clay intervening between the two terraces.

The chief points of interest which the author submitted to the judgment of geologists, consisted in :—the presence of drift implements in the high terrace, their absence in the mid terrace, and reappearance in the existing bed of the Thames; the great rarity or absence of animal remains in the high terrace, and their abundance in the mid terrace; and the occurrence of both implements and animal remains at the bottom of the gravel in both terraces. The writer concluded by adducing proofs of the great antiquity of the present river-bed, which it was shown must have run in its present meandering course in the bottom of the valley for at least 2000 years.

3. " On the Animal Remains found by Col. Lane Fox in the High- and Low-level Gravels at Acton and Turnham Green." By George Busk, Esq., F.R.S., F.G.S.

The author described the mammalian bones referred to in the preceding paper.

The remains from the High-level Gravels at Acton belong to the genera *Bos, Ovis, Equus,* and *Elephas*? The greater part belong to the first-named genus, and are probably modern, as are also those of *Ovis*. The remains of *Equus* may be of greater antiquity. The other bones found may belong either to Elephant, Rhinoceros, or Hippopotamus; they include a large portion of an Elephant's molar, and are much rolled.

The remains from the mid-level gravel at Turnham Green generally present the characters of great antiquity. They include bones of *Rhinoceros hemitœchus, Equus caballus, Hippopotamus major* (one of them the left frontal of a very young animal almost unworn), *Bos* (probably *B. primigenius,* and some perhaps *Bison priscus*), *Cervus* (*C. clactonensis,* Falc.= *C. Browni* Dawk., *C. elaphus,* and *C. tarandus*), *Ursus ferox priscus,* and *Elephas primigenius.*

4. " On the Evidence for the Ice-sheet in North Lancashire and adjoining parts of Yorkshire and Westmoreland." By R. H. Tiddeman, Esq., M.A. Oxon, F.G.S., of the Geological Survey of England and Wales.

The country of which the earlier glacial phenomena were de-

scribed in this paper lies between the Lake-district on the north and the plains of South Lancashire and Cheshire on the south, and extends from the great watershed of England to the Irish Sea.

On the west is a sea-side plain rising to levels of less than 200 feet. On the north-east is a portion of the Pennine chain, comprising Ingleborough, Pennigent, and other Fells, rising to heights of from 2000 to 2400 feet. Between these, from south to north, we pass over:—1, a range of moorlands from 1000 to 1500 feet high, called the Rossendale Anticlinal, which forms the watershed between the basins of the Mersey and the Ribble; 2, the valley of the Burnley and Blackburn Coal-field, which drains north through gorges in (3) the Pendle chain of hills into (4) the broad valley of the Ribble; 5, a group of Fells rising to a general level of 1800 feet, between the valleys of the Ribble and the Lune, called, for the purposes of this paper, " The Central Fells;" 6, north of this the valley of the Lune and the estuary of the Kent. The main direction of all these features, between the sea-side plain and the Pennine chain, is from north-east to south-west.

The paper was illustrated by a map of the district on the scale of 1 inch to a mile, coloured to represent elevations, the level-contours having been reduced from the 6-inch scale. Upon this all the ice-scratches found on the solid rocks were inserted. A diagram illustrating the proportional number of scratches in different directions showed that 20 per cent. of them were due south, although the general direction of the valleys was to the south-west.

An instance was mentioned of a ridge 1400 feet in height, which had scratches at the top running directly across it to the south, although no land of equal height occurred north of it within a distance of 7 miles. A similar instance was shown to exist on the ridge north-east of Pendle Hill. A *roche moutonnée* in the gorge of the Calder at Whalley was shown to have been formed by ice working from the north, although the river drains from the south. Other systems of scratches were mentioned in detail. All these tended to show that, though the general slope and drainage of the district is to the south-west, the movement of the ice at the period of maximum cold was to the S. or S.S.E., or nearly parallel to the watershed.

The author goes on to describe certain disturbances at the surface of the rocks which dip at high angles to the south, they having been overturned by some force coming from the north. Such surface-disturbances are not found on rocks dipping to the north; and this fact may be explained by an illustration : in one case the brushing was with the nap, in the other against it. It was shown that these phenomena could not be attributed to any other agent but a great ice-sheet pushing on from its northern gathering-grounds, recruited by the greater elevations on its course, but overriding the lesser, grinding down and smoothing by its friction rocks presenting but a gentle incline, tearing up and turning over the basset edges confronting its approach.

The author next described the arrangement of the Till as to colour and material, and endeavoured to show that all the facts

which he has observed are in favour of the existence of an ice-sheet travelling south in this district.

Mr. Cumming's observations in the Isle of Man were considered to confirm these views. He describes the general glaciation of the island as being from the E.N.E. or Lake-country, and describes many large blocks of granite which had been carried from their parent rock up the high hill of South Barruh and down the other side. This was referred by Mr. Cumming at the time to a great "wave of translation;" but the facts are quite easily explained by an ice-sheet. Other observations of Mr. Cumming upon the drifts of the Isle of Man were taken by the author as confirmatory of his views. Mr. Morton's observations on the glaciation of the Mersey basin were touched upon; and it was suggested that the glaciation of that district was produced by an ice-sheet, not coming from the south-east, as Mr. Morton holds, but working to the south-east from the Lake-country, and across a part of what is now the Irish Sea.

Professor Ramsay's observations on the glaciation of Anglesey being to the S.S.W. instead of from the Snowdon group, as might have been expected, were considered by the author to be confirmatory of his views of a great ice-sheet having filled what is now the Irish Sea, and emptied itself by St. George's Channel on the one hand, and by the Cheshire plain on the other, as well as by some of the passes in the Pennine Chain.

5. "On the Mammalia of the Drift of Paris and its Outskirts." By Prof. Albert Gaudry, F.C.G.S. (In a letter to W. Boyd Dawkins, Esq., M.A., F.R.S., F.G.S.)

In this paper the author briefly indicated those mammals the remains of which have been discovered in the Pleistocene or Quaternary deposits of Paris and its vicinity. His list includes flint implements as evidences of the existence of man, and bones of the following species:—*Canis lupus, Hyæna crocuta (spelæa), Felis leo (spelæa), Castor trogontherium* and *fiber, Elephas primigenius* and *antiquus, Hippopotamus amphibius, Rhinoceros tichorhinus* (a Rhinoceros of doubtful species), *Sus scrofa, Equus asinus* and *caballus, Bos primigenius, taurus*?, and *indicus*?, *Bison priscus* and *europæus,* and *Cervus tarandus, Belgrandi, megaceros, canadensis*?, *elaphus,* and a small species.

XXIX. *Intelligence and Miscellaneous Articles.*

ON THE ACTION OF A CONDUCTOR ARRANGED SYMMETRICALLY ROUND AN ELECTROSCOPE. BY CH.-V. ZENGER.

I HAVE the honour to address to the Academy the result of some fresh experiments on the electric inertia of a conductor arranged symmetrically round an electroscope.

Ruhmkorff found that if static electricity exercises no action on the electroscope disposed as I have indicated, it is not so with dynamic electricity or the electricity of induction.

This result is only a confirmation of my theory of electric inertia, since the condition of equal distribution (equal superficial tension), and symmetrical, is not fulfilled when induction-apparatus is used. In fact the tension of the current after the opening and after the closing of the inducing current is not the same, and the charge of the symmetrical conductor is successively positive and negative; the superficial tension cannot be none, nor even equal, since a certain time is required for the two electricities to combine after two alternate unequal discharges, considering the tension and the nature of the electricity. The tension at the part of the symmetrical conductor furthest from the point of discharge will be quite different from that at the part nearest to the conductor of the Ruhmkorff; and the condition of equal superficial tension at every point of the symmetrical conductor is not fulfilled. Failing this essential condition, there will be an action nearly equal to the difference of tension of the sparks of opening and of closing.

To show the influence of the symmetrical distribution, I put symmetrically round a gold-leaf electroscope a rectangular copper wire : the electroscope and the wire are placed upon the brass plate of another electroscope (with straws instead of gold leaves), larger and less sensitive. From the conductor of an electrical machine strong sparks go to one of the angles of the wire; the upper electroscope shows not a trace of tension, while the straws of the large electroscope below are strongly affected.

If the experiment be modified by placing the knob of the upper electroscope not symmetrically in relation to the middle points of the sides of the conductor, there will be seen a movement of the gold leaves at every discharge from the conductor of the machine. The greater this defect of symmetry, the more sensible will be the action.—*Comptes Rendus de l'Académie des Sciences*, vol. lxxv. p. 1765.

ON THE HEAT OF TRANSFORMATION.   BY M. J. MOUTIER.

A substance may present itself at the same temperature in two different states, which we will call M and M'. In passing from M to M' a kilogramme of the substance absorbs a quantity of heat Q, which we will call *heat of transformation*. Let us suppose that in both conditions the substance can be vaporized, and that the tensions p and p' of the vapours at the same temperature are unequal; we propose to determine a relation between the heat of transformation Q and the tensions p and p'.

Let u and u' be the specific volumes of the substance in the states M and M', v and v' the specific volumes of the vapours given by M and M', L and L' the heats of vaporization.

Let us conceive the following cycle of operations effected at a constant temperature :—

(1) The substance passes from the state M to M' under a constant pressure π; it absorbs the quantity Q of heat. The quantity of heat consumed by external work is $A\pi(u'-u)$, A being the heat-

equivalent of the work; the heat expended on internal work is $Q - A\pi(u'-u)$.

(2) The body M' is reduced to saturated vapour. The heat consumed by external work is $Ap'(v'-u')$; that consumed by internal work is $L' - Ap'(v'-u')$.

(3) The temperature being kept constant, the volume of the vapour formed is changed so that the pressure becomes equal to p; the internal work consumes the quantity q of heat.

(4) We condense the vapour under the constant pressure p; the body returns to the initial state M. The heat disengaged is L, of which the portion corresponding to the internal work is $L - Ap(v-u)$.

The cycle is closed; the change of internal heat is *nil*:

$$Q - A\pi(u'-u) + L' - Ap'(v'-u') + q - [L - Ap(v-u)] = 0 ;$$
$$Q = L - L' + A\pi(u'-u) + Ap'(v'-u') - Ap(v-u) - q. \quad . \quad (1)$$

If we neglect the volumes u and u', if we assume that the internal work in the transformation of the vapour during the second operation is insensible, and, further, that the vapour follows Mariotte's law

$$pv = p'v', \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

we obtain the relation, in general sufficiently approximate,

$$Q = L - L'. \quad . \quad . \quad . \quad . \quad . \quad (3)$$

The heat of transformation is equal to the difference of the heats of evaporation.

According to Carnot's theorem, if T denotes the absolute temperature of the body,

$$L = AT(v-u)\frac{dp}{dT}, \;\; L' = AT(v'-u')\frac{dp'}{dT}. \quad . \quad . \quad (4)$$

Taking into consideration relation (2), and neglecting u and u' in comparison with v and v', we get another expression—

$$Q = ATvp\,\frac{d}{dT}\log\left(\frac{p}{p'}\right), \quad . \quad . \quad . \quad . \quad (5)$$

where log denotes a Napierian logarithm.

Let us apply these relations to a few examples.

Solution.—Let us consider a kilogramme of water and a sufficient weight of salt to give a saturated solution at the temperature T. Pure water has a vapour-tension p; that of the saturated solution is p'. The quantity of heat absorbed by the solution of the salt in the water is given by the preceding relations. Relation (5) was first pointed out by Kirchhoff [*].

Fusion.—We owe to M. Regnault a series of " Researches undertaken for the purpose of deciding if the solid or liquid state of

* *Journal de Physique théorique et appliquée*, vol. i. p. 30.

bodies exercises an influence on the elastic force of the vapours emitted by them *in vacuo* at the same temperature." The conclusion deduced from M. Regnault's experiments is the following :— "that the molecular forces which determine the solidification of a substance do not exercise any sensible influence on the tension of its vapour *in vacuo*; or, more exactly, if an influence of this kind exists, the variations it produces are so slight that they could not be certainly established in our experiments." It must, however, be added that monohydrated acetic acid forms an exception; Regnault attributes this anomaly to impurity of the acid.

Let us see what the preceding theory indicates in this case; we have to do with very small differences. We will take the exact formula*. Suppose the vapour-tensions equal in the two conditions, $p = p'$; then $v = v'$, $q = 0$, and, taking into consideration equations (4),

$$Q = (u' - u)A \left(T \frac{dp}{dT} + \pi - p \right).$$

Take as an example the melting of ice at zero under the pressure of the atmosphere. The substance M represents the ice at zero; M', the liquid water at zero. The ice in melting absorbs heat; Q is positive, $u' - u$ negative, the parenthesis positive; therefore the hypothesis $p = p'$ conducts to an inadmissible conclusion.

Thus *ice and liquid water*, both at zero, *have different vapour-tensions*. The difference is very small; the approximate formula (5) shows it readily. The term $ATvp$ is very considerable in comparison with Q, so that p and p' differ very little. This result is entirely conformable to the latter part of M. Regnault's conclusion.

Allotropy.—According to the experiments of MM. Troost and Hautefeuille[+], ordinary phosphorus at the temperature of 360° has a vapour-tension $= 3\cdot2$ atmospheres. This vapour, under the prolonged action of heat, deposits red phosphorus; and the transformation ceases when the tension of the vapour takes the minimum value 0·6 atm., which MM. Troost and Hautefeuille have named *tension of transformation*. This minimum tension may be regarded as the maximum tension of the vapour of red phosphorus at the temperature of the experiment. Supposing, then, white phosphorus to correspond to the first state, M, red phosphorus to the second, M', we have

$$T = 273 + 360, \quad p = 3\cdot2 \text{ atm.}, \quad p' = 0\cdot6 \text{ atm.};$$

according to MM. Troost and Hautefeuille, at 440°, or

$$T = 273 + 440, \quad p = 7\cdot5 \text{ atm.}, \quad p' = 1\cdot75 \text{ atm.}$$

The weight of the litre of phosphorus-vapour which remained in the state of vapour at 360° after 240 hours of heating was 1·4 gramme; therefore the specific volume of the vapour of red phosphorus at

that temperature is

$$v' = \frac{1}{1 \cdot 4} \text{ cubic metre.}$$

If we apply these data of experiment to formula (5), taking into consideration equation (2), we find, for an approximate expression of the heat of transformation of white into red phosphorus,

$$Q = -17 \cdot 5.$$

Thus, as M. Favre announced a little while after M. Schroetter's discovery, white phosphorus disengages heat in passing into the condition of red phosphorus. From an experiment by M. Hittorf, the transformation of liquid white phosphorus at 280° determines a sudden rise of the temperature from 280° to 370°. Designating by c the specific heat of the phosphorus the temperature of which thus rises, we should have $c \times 90 = 17 \cdot 5$; from this we deduce $c = 0 \cdot 19$, a number which differs little from the specific heat found by M. Regnault.—*Comptes Rendus de l'Acad. des Sciences*, vol. lxxvi. pp. 365–368.

ROYAL ASTRONOMICAL SOCIETY.

We were not a little disappointed on attending the Anniversary Meeting on the 14th of February to find that the Medal had not been awarded for the current year. The failure of the Council to find an astronomer whose attainments are such as to entitle him to become the recipient of the highest honour the Society has the power to bestow, suggests some important queries relative to the actual state of astronomy at the present time on the one hand, or to the condition of the Society which is, or ought to be, the representative of the science in England on the other. If we remember rightly, *one* medal only has been awarded during the last *three* years, and that to a foreign astronomer well deserving of it. Is there no English astronomer on whom the Society could gracefully and legitimately bestow it? Are the claims of our American brethren exhausted? or can no continental astronomer be found worthy to swell the list of medallists of the Society by the reception of its mark of highest approbation for services well and faithfully performed? We shall not attempt to reply to these queries. Those distinguished astronomers who take the lead in guiding astronomic thought in this country are well able to answer them. If, however, only one astronomer could be found (and we want but one annually) to whom the presentation of the Medal would alike confer honour on the Society and on the recipient, we should by the award take note of the continued onward progress of the Science; but as it is, we are in doubt as to whether astronomy is declining, or whether it is duly represented by the Society, which hitherto has held astronomical prestige in its hands. Of late years we have looked in vain for the choice spirits who held rule and sway at Somerset House when Baily, Sheepshanks, Herschel, and others sat in council; but few such spirits are now left to reflect the former

glory of the Society ; and the question remains, Is the Society now
so constituted that it represents the true state of astronomy in
England, and is the administration of its affairs such as to en-
courage, stimulate, and reward the patient worker in the humbler
ranks of observers, and the veteran who has won his laurels which
would gather lustre by the award of the Medal ?

ON THE DETERMINATION OF THE BOILING-POINT OF LIQUEFIED SULPHUROUS ACID. BY M. IS. PIERRE.

In his very interesting memoir on sulphurous and chlorhydric
acids (*Comptes Rendus*, Jan. 13, 1873), M. Melsens says that from
1860 he has been seeking to determine the exact boiling-point of
liquefied sulphurous acid, that he has made very numerous trials
with vessels of all sorts, but that all his attempts have been fruit-
less. Nevertheless, if content with an approximation to $0°\cdot15$ or
$0°\cdot2$, it is very easy to determine the boiling-point of liquid anhy-
drous sulphurous acid by following the process which I pointed
out twenty-six years since (*Annales de Chimie et de Physique*, ser. 3,
vol. xxi.), in a memoir on sulphurous acid. This consists in pour-
ing into a tube of *thin* glass, 2·5 to 3 centims. in diameter, having
the form of a test-tube for gases, a certain quantity of sulphurous
acid previously cooled—fitting to the aperture a cork pierced with
two holes, one to give passage to the thermometer, the other
larger, intended to give free passage to the vapour of the acid by
means of a rather wide tube of thin glass—and, lastly, suspending
the apparatus in the air. This is what then takes place :—The
surrounding temperature being above that at which sulphurous
acid boils, the latter is very soon in ebullition ; but the heat ren-
dered latent by its vaporization lowers the temperature of the re-
maining liquid, and produces an abatement of the ebullition. This
is soon followed by a renewal ; and thus a series of abatements and
renewals of ebullition is observed, during which the differences of
temperature indicated by the thermometer rarely reach $0°\cdot2$.

The limits are still more contracted when the deposition of mois-
ture on the tube is avoided by covering with flannel the part con-
taining the liquid. With from 25 to 30 grammes of liquid, if the
operation takes place under favourable conditions, the experiment
may often last more than an hour. I have constantly repeated it
in my lectures for the last twenty-five years, on account of its
facility.

I have thus found a number which differs very little from $8°$
below zero (Centigrade). The process, extremely simple, is appli-
cable to all liquefied gases which can be kept in an *open* vessel,—
that is to say, which in a certain time emit by ebullition a quantity
of vapour that absorbs and renders latent an amount of heat equal
to that received by the liquid from the surrounding medium—a
condition from which results a temperature of spontaneous ebulli-
tion sensibly constant.—*Comptes Rendus de l'Académie des Sciences*,
Jan. 27, 1873.

XXX. *On Spectral Lines of Low Temperature.*
By The Marquis of SALISBURY, *F.R.S.*[*]

IF one secondary pole of a powerful inductorium be connected
with an insulated metal plate, the other pole being left un-
connected, and a thermometer be fixed upright upon the plate, a
green light will be visible in the vacuum above the mercury.
In order to obtain the effect at its best, the battery should be
slightly stronger than is necessary to produce the maximum
spark between the secondaries of the coil used; and the plate
should be completely insulated.   By what kind of electric
action this light is produced is not quite clear.   Plücker and
Mr. Gassiot, and others following them, speak of a similar light
produced in a closed tube, without wire electrodes, as being
caused by induction.   The process appears to me more nearly
to resemble conduction, the circuit being completed by leakage.
At the point where the bulb rests upon the plate a discharge is
visible, oxidizing the plate.   At the other end of the thermo-
meter a brush-discharge may (in the dark) be seen escaping.   If
a metal conductor be supported vertically parallel to the ther-
mometer, with a slight interval between itself and the plate, and
insulated at the other end, a similar discharge and similar,
though more abundant, escape will be visible.   If a piece of wax
be inserted between the metal conductor and the plate, the re-
semblance will be closer still.   As long as the conductor is
there, the light in the thermometer will not appear, or will
appear only by flashes.   When the conductor is removed, the
light returns to the thermometer.   It appears, therefore, that

* Communicated by the Author.

the electric action on the thermometer is of the same kind as the action upon the conductor.

But whatever the process of electric excitement in the thermometer may be—whether by induction or by conduction through leakage—the exhibition of light without any but the minutest development of heat is worthy of notice. The heat caused by a full discharge through Geissler's tubes is well known. It will, as Wüllner notices, disintegrate the glass, producing sodium-, and at last even calcium-lines. If increased by resistance of gas of 500 millims. pressure, it will soften and bend aluminium electrodes. But in the thermometer, where the resistance is that of the thickness of a tube of glass, scarcely any, if any, rise of temperature is produced. With thermometers of the ordinary bore, I have failed, after many trials, to discover any alteration of reading at the end of five minutes' exposure to the discharge of the coil in the manner I have described. Four experiments with a thermometer of very fine bore, graduated to tenths of a degree Fahr., have given an average rise of three quarters of a degree Fahr. in the course of five minutes. I am doubtful whether even this rise is to be attributed to heat, and for this reason. In two other thermometers a small portion of the column of mercury was separated from the rest. The action of the coil after a short time made the interval wider, pushing up the separated fragment without expanding either portion; and in the interval so created a brilliant light appeared. It is evident, therefore, that the coil tends to produce a motion in the mercury of the thermometer analogous to that observed by Poggendorff in larger tubes; and the slight apparent rise of three quarters of a degree in five minutes, produced by the coil in a thermometer of very small bore, may be due to an action of this kind. But even if there be a real rise of temperature to this extent, it is so minute as to be practically unimportant. In effect, therefore, the light was produced at a temperature of less than 60° F.

This light is strong enough to be examined in the spectroscope. One prism, of course, only must be used; and the room must be perfectly dark. It is also necessary to use cross-wires in the telescope, as cross-hairs are too slender to be seen in so dim a light. Of course much the easiest mode of examination is to dispense with a slit altogether; but on that plan the lines of the spectrum, in some thermometers at least, are so rugged that it is difficult to identify them. I have therefore been compelled to use the slit. To my surprise, I found that different thermometers gave different lines; or rather, some gave more lines than others. Instruments made by the best makers, such as Casella or Elliott, give only the three following lines. All the

lines observed have been identified by means of comparison with Geissler's tubes showing known spectra. The wave-lengths of the lines in the tubes so used for comparison I afterwards measured with a grating, and verified by Dr. Watts's catalogue. The light of the thermometer itself is too feeble to permit of direct measurement with a grating.

				$\lambda =$	Dr. Watts's No.
$a\,a$	.	.	orange	5768	53·6
$c\,c$	.	.	green	5460	63·3
$h\,h$	.	.	violet	4352	124·2

These are three of the brightest mercury-lines. The tube of comparison was one in which, previously to exhaustion, some mercury had been vaporized. I mention the grounds of my conclusion, because in Wüllner's experiments it was shown that mercury required considerable heat before its lines could be made to appear (Pogg. *Ann.* vol. cxxxv. p. 512). No one, however, who examines these lines will doubt that they are mercury-lines; and they certainly are produced without any appreciable rise of temperature. They appear in every thermometer I have examined. There was a fourth line ($d\,d$) in one of Casella's thermometers, apparently coincident with mercury-line No. 84·8 of Dr. Watts; but it was so feeble that the cross-wires could not be brought upon it with any certainty.

But in chemical thermometers purchased from six makers of slightly less repute than those above named, four other lines made their appearance in addition. These do not coincide with the mercury-lines, nor with the lines of a hydrogen-tube, nor with the lines of air; but they coincide exactly with some of the strongest lines of tubes containing carbon compounds. I find them in carbonic acid, in paraffin-oil vapour, in hydrocyanic acid, in cyanogen, in olefiant gas, in methyl, in coal-gas, and in carbonic oxide. Their wave-length has been verified with a grating as before.

Thermometer-line.		Dr. Watts's carbon spectra.	λ.
$b\,b$	(j)	58, yellow	5602
$e\,e$	(k)	74, green	5195
$f\,f$	(l)	92, blue	4834
$g\,g$	(m)	112, violet	4505

Many observers have been plagued by the intrusion of carbon-lines in various tubes where they had no place. Wüllner even assigns to carbon one of the various spectra which oxygen-tubes have been found to present. These carbon-lines are attributed by him to the grease surrounding stopcocks; by others (I think, by Mr. Crookes) to caoutchouc joints. These explanations are clearly not applicable to thermometers; and I think a

simpler one, at least in some cases, may be offered. Every photographer must have observed the singular power which glass, exposed to an inhabited atmosphere, possesses of attracting not only moisture, but also thin films of grease. If a photographic glass plate be thoroughly cleaned and put aside for some days, it can very seldom be used at the end of that time without further treatment. Unless it be cleaned afresh with some solvent of grease, such as ether or strong acid, or rubbed with tripoli, it will probably show on the finished picture the streaks which betoken an imperfectly cleaned plate. What is true of a glass plate is probably true of the inner surface of a tube. Unless it is cleaned with something more powerful than water, it is pretty certain to retain a film of grease. I interpret these carbon-lines, therefore, as a simple proof of slovenly preparation. Their presence or absence may serve as an easy test, not of the skill, but of the care with which the thermometer has been made. I find that it depends more upon the reputation of the maker than on the price charged for the instrument. One of Elliott's that showed only mercury-lines is a common two-shilling instrument. In one costly, but apparently very dirty, thermometer—besides the four carbon-lines, one or two other very faint green lines showed themselves in the part of the spectrum near the solar line E; but they were too faint to be identified. In this thermometer, after frequent exposure to the action of the coil, a slight grey deposit was observable in the tube just over the point where the mercury ordinarily stood.

But if these carbon-lines show grease, why does the hydrogen present in grease not show its characteristic lines, especially the line F, which is the first seen of the hydrogen-lines? I believe the reason to be that hydrogen will not become luminous under electric influence at a low temperature, while the vapour of (combined) carbon will. This may be shown in a very simple way. Place a Geissler's tube, exhausted from some hydrocarbon compound (paraffin oil answers well), upright on the insulated plate connected with one secondary pole. Let a wire in connexion with the other pole be so arranged that it can be at will applied to or withdrawn from the upper end of the Geissler's tube while the coil is at work. First apply it, and let the full discharge pass. The line F and the carbon-line I have named ff will be seen close together with a deep-black space between them, giving the effect (in a one-prism instrument) of a groove with luminous edges. Withdraw the movable electrode; F will immediately disappear; but the carbon line ff will remain, little diminished in brightness. Approach the movable electrode to the top of the tube until a spark occasionally passes. Whenever the spark passes, the line F will shoot across the spectrum; but as long as there is no

spark, only the carbon-lines will be visible.   It appears to follow
that the specific luminosity of carbon under electric excitement
is much higher than that of hydrogen.   If the light in a Geiss-
ler's tube be really produced exclusively by heat, as is commonly
assumed, it follows that carbon at low pressure becomes lumi-
nous under the influence of very little heat.   It is worth noting
that with a broken circuit, such as I have described, the carbon-
lines are distinct lines equally sharp on both sides.   With a full
circuit they are (in the prism) sharp only on one side, the least
refrangible ; on the other they show an expansion, a tendency to
melt into the obscure continuous spectrum that lies behind them ;
on this side they much resemble the hydrogen-lines of the third
spectrum, produced by intense heat.   In short, the carbon-lines
are visible when the hydrogen-lines have not yet appeared ; the
carbon-lines have begun to expand while the hydrogen-lines still
remain sharp.   Both substances pass through the same stages
in their passage to the continuous spectrum to which all gases,
by heat and pressure, can be ultimately reduced ; but carbon
begins the process first, and probably finishes it first ; for it may
be inferred that the spectrum of carbon vapour reaches the con-
tinuous condition proportionally sooner.   I think I have seen it
argued that there can be no carbon gases in the sun, because
there are no carbon-lines.   It has certainly been assumed that
the carbon in a candle-flame must be in the solid state, because
the spectrum is continuous.   The above observations may tend
to throw doubt on both these positions.   If this carbon-spectrum
is visible at a low temperature, it may reach the continuous con-
dition, and consequently not be recognizable, at a temperature
lower than the flame of a candle.   Of course, if all carbon-spectra
implied a similar temperature, the existence of the Bessemer-
spectrum would negative such an idea.   But Dr. Watts has
shown that this is far from being the case.

I do not know whether it would be possible, by slight admix-
tures with the mercury of a thermometer, to ascertain the lowest
temperature of luminosity in the case of other elements ; but
the investigation would be worth making if it is practicable.

XXXI. *On an advantageous Method of using the Differential Gal-
vanometer for measuring small Resistances.*   By OLIVER
HEAVISIDE, *Great Northern Telegraph Company, Newcastle-
on-Tyne*[*].

IN the usual method of measuring resistances with the differen-
tial galvanometer, the current from the battery is divided
between the two coils, having opposite effects on the needle

within them, so that, if the currents in both the coils are equal, the needle is unaffected. The introduction of resistance in the circuit of one coil will not affect the balance, provided an equal resistance is introduced in the circuit of the other coil. Hence, if on one side we place a rheostat, and on the other an unknown resistance, the latter may be determined by varying the resistance of the rheostat until a balance is obtained. Fig. 1 is a representation of this arrangement. The current from the battery, having a resistance b and electromotive force E, divides at the point a between the coil g and resistance x, and the coil g' and resistance r. When $r = x$, the needle is unaffected.

Fig. 1.

By the following method of using the differential galvanometer, a much greater accuracy is obtained when the unknown resistance whose value has to be determined is small.

Instead of dividing the current from the battery E between the two coils, I join up the coils, so that the same current passes through both of them, and by reversing one of the coils, g', prevent the current from influencing the needle (see fig. 2). The rheostat r is connected to the two ends of one coil, and the resistance to be measured, x, to the two ends of the other. It will easily be seen, without further explanation, that when $r = x$, the currents in g and g' are equal; but should r not equal x, there will be a greater current in one coil than in the other, and the needle will move in obedience to the difference

Fig. 2.

of these currents. It then only remains for me to show what, and under what circumstances, advantages are obtained by this method. To do so, we have only to compare the expressions for the difference-currents in the two methods.

By the first method the resistance external to the battery is

$$\frac{(x+g)(r+g)}{x+r+2g};$$

therefore the current from the battery is
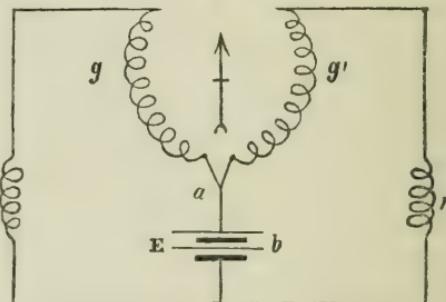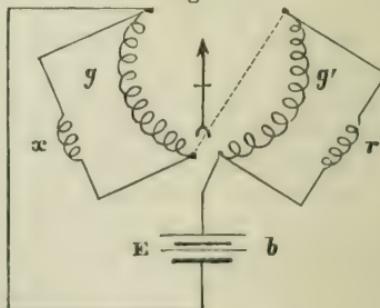
$$B = \frac{E}{b + \dfrac{(x+g)(r+g)}{x+r+2g}} = \frac{E(x+r+2g)}{b(x+r+2g) + (x+g)(r+g)}.$$

This current divides between the two paths $x+g$ and $r+g$ in inverse proportion to their resistances; therefore the current in g is

$$G = B \times \frac{r+g}{x+r+2g} = \frac{E(r+g)}{b(x+r+2g) + (x+g)(r+g)},$$

and the current in g' is

$$G' = B \times \frac{x+g}{x+r+2g} = \frac{E(x+g)}{b(x+r+2g) + (x+g)(r+g)}.$$

The effective current (that influencing the needle) will be the difference of G and G', say

$$D_1 = \frac{E(r-x)}{b(x+r+2g) + (x+g)(r+g)}. \quad . \quad . \quad . \quad (1)$$

By the second method, the resistance external to the battery is

$$\frac{xg}{x+g} + \frac{rg}{r+g};$$

therefore the current from the battery is

$$B = \frac{E}{b + \dfrac{xg}{x+g} + \dfrac{rg}{r+g}} = \frac{E(x+g)(r+g)}{b(x+g)(r+g) + gx(g+r) + gr(g+x)}.$$

The current in g is

$$G = B \times \frac{x}{x+g} = \frac{Ex(g+r)}{b(x+g)(r+g) + gx(r+g) + gr(x+g)},$$

and the current in g'

$$G' = B \times \frac{r}{r+g} = \frac{Er(g+x)}{b(x+g)(r+g) + gx(r+g) + gr(x+g)}.$$

The effective current will therefore be

$$D_2 = G - G' = \frac{Eg(x-r)}{b(x+g)(r+g) + gx(r+g) + gr(x+g)}. \quad . \quad (2)$$

Equations (1) and (2) give the effective current in each case; and we may ascertain the relative sensitiveness of the two methods by comparing D_1 and D_2.

$$\frac{D_2}{D_1} = \frac{b(x+r+2g) + (x+g)(r+g)}{b(x+g)(r+g) + gx(r+g) + gr(x+g)} \times g;$$

and in the limit, when $x = r$,

$$\frac{D_2}{D_1} = g \times \frac{2b + r + g}{b(r + g) + 2gr}.$$

When $r = g$, $\dfrac{D_2}{D_1} = 1$, showing that the two methods are equally sensitive for that value of r or x which equals the resistance of one coil of the galvanometer. When r is greater than g, the ordinary method is to be preferred, for $\dfrac{D_2}{D_1}$ is then less than unity. It can, however, never be less than $\dfrac{g}{b + 2g}$, which happens when r is infinite.

But for values of r less than g, $\dfrac{D_2}{D_1}$ is greater than unity, and increases rapidly as r is reduced, until in the limit, when $r = 0$, $\dfrac{D_2}{D_1} = 2 + \dfrac{g}{b}.$

This proves that when the resistance to be measured is smaller than that of the galvanometer-coil, the second method is much to be preferred. For instance, let the battery have a resistance of 10 ohms, the galvanometer (each coil) 500 ohms, and $r = 10$ ohms, then the second method is 17 times as delicate as the first; and if r were 1 ohm, the second method would be 416 times as delicate.

In fact, if, after getting as true a zero as possible by the ordinary method, the connexions be altered to the second arrangement, the slight inequality between r and x, which was inappreciable by the ordinary method, will be at once rendered evident by a large deflection of the needle.

XXXII. *On the Optics of Mirage.* (Second Paper.) *By* Professor EVERETT, *M.A., D.C.L., Queen's College, Belfast**.

XI. WE proved in the preceding paper that wherever a plane of maximum index exists, the surfaces-of-equal-index being parallel planes, the law of index-variation in its immediate neighbourhood must in general be

$$\frac{d \log \mu}{dy} = -\frac{y}{a^2},$$

and that this law implies the existence of conjugate foci at the mutual distance πa.

It may be proved in like manner that, when the surfaces-of-

equal-index are circular cylinders having for their common axis a line of maximum index, the law of index-variation in the immediate neighbourhood of this axis must in general be

$$\frac{d \log \mu}{dr} = - \frac{r}{a^2},$$

r denoting distance from the axis. Hence it is clear, from Sections II. and III. of the preceding paper, that rays diverging, at small inclinations to the axis, from any point in or near it, and lying in one plane which also contains the axis, will converge to a series of conjugate foci, whose common distance measured parallel to the axis is πa. We shall now show that this property is not confined to rays lying in the same plane with the axis, but extends to all rays of small inclination to the axis.

Employing rectangular axes of x, y, z, and making the axis of x coincide with the line of maximum index, it follows[*], from the supposed smallness of the inclination of the rays to this line, that $\frac{d^2x}{ds^2}$ is negligible in comparison with the curvature $\frac{1}{\rho}$, and that, to the same degree of approximation,

$$\frac{d^2y}{ds^2} = \frac{d^2y}{dx^2}, \quad \frac{d^2z}{ds^2} = \frac{d^2z}{dx^2}.$$

The radius of curvature ρ is therefore given by the equation

$$\frac{1}{\rho^2} = \left(\frac{d^2y}{dx^2}\right)^2 + \left(\frac{d^2z}{dx^2}\right)^2;$$

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[*] Let α denote the inclination of the ray, at point x, y, z, to the axis of x; then we have

$$\frac{d^2x}{ds^2} = \frac{d}{ds}\frac{dx}{ds} = \frac{d}{ds}\cos \alpha = - \sin \alpha \frac{d\alpha}{ds}.$$

But $\frac{d\alpha}{ds}$ is of the same order of magnitude as $\frac{1}{\rho}$; hence $\frac{d^2x}{ds^2}$ is very small in comparison with $\frac{1}{\rho}$—that is, with the square root of

$$\left(\frac{d^2x}{ds^2}\right)^2 + \left(\frac{d^2y}{ds^2}\right)^2 + \left(\frac{d^2z}{ds^2}\right)^2.$$

We may therefore write

$$\frac{1}{\rho^2} = \left(\frac{d^2y}{ds^2}\right)^2 + \left(\frac{d^2z}{ds^2}\right)^2.$$

The general expression for $\frac{d^2y}{ds^2}$ is

$$\frac{d^2y}{dx^2}\left(\frac{dx}{ds}\right)^2 + \frac{dy}{dx}\frac{d^2x}{ds^2},$$

of which the first term is, in the present case, equal to $\frac{d^2y}{dx^2}$, and the second vanishes.

and its direction-cosines (regarding it as drawn *from* the centre of curvature) are

$$0, \quad -\rho \frac{d^2 y}{dx^2}, \quad -\rho \frac{d^2 z}{dx^2}.$$

Now the line r is in the osculating plane of the ray at the point x, y, z, because the osculating plane always contains the direction of most rapid change of index; hence the lines r and ρ are coincident, their common direction being defined by the intersection of the osculating plane with a plane perpendicular to the axis of x. Their direction-cosines are therefore equal; that is,

$$\frac{y}{r} = -\rho \frac{d^2 y}{dx^2}, \quad \frac{z}{r} = -\rho \frac{d^2 z}{dx^2}.$$

But we have

$$\frac{1}{\rho} = -\frac{d \log \mu}{dr} = \frac{r}{a^2}.$$

Therefore

$$\frac{d^2 y}{dx^2} = -\frac{y}{r}\frac{1}{\rho} = -\frac{y}{a^2}; \quad \frac{d^2 z}{dx^2} = -\frac{z}{a^2}.$$

But it is clear, from Section II. of the previous paper, that the general differential equation of a ray in the plane xy is $\dfrac{d^2 y}{dx^2} = -\dfrac{y}{a^2}$. Hence the projection of a ray upon the plane xy is identical with the path of a ray in the plane xy; or, to state the same thing in general terms, the projection of a ray upon any plane containing the axis of x is identical with the path of a ray in this plane.

The equations of the projections of a ray upon the planes xy, xz are

$$y = b \sin \frac{x-c}{a}, \quad z = b' \sin \frac{x-c'}{a},$$

b, c, b', c' being arbitrary constants. They show that the ray is in general a helicoid or flattened helix, capable of being inscribed in a rectangular tube whose section has the length and breadth $2b$ and $2b'$.

As the substitution of $x + \pi a$ for x merely changes the signs of y and z, every object in the neighbourhood of the axis of x will yield a series of real images alternately inverted and erect, their common distance apart measured parallel to the axis of x being πa. The pencil of rays proceeding from any point to its conjugate will in general consist of helicoids, both right-handed and left-handed, of every degree of flatness, from the true helix to the plane curve of sines. If the point is on the axis of x, all the rays will be plane; and even if the surfaces-of-equal-index be not circular cylinders, provided only they be surfaces of revolution about the axis of x, rays emanating from a point on this

axis will converge to a geometrical focus also situated upon it, as proved (for rays in one plane) in the previous paper.

XII. The path of a ray under the conditions treated in last section is the same as that of a particle attracted towards the axis of x by a force varying directly as the distance. For, let the intensity of force at distance r be $\dfrac{r}{a^2}$; then the components parallel to the axes of y and z will be $\dfrac{y}{a^2}$ and $\dfrac{z}{a^2}$; so that the differential equations of motion will be

$$\frac{d^2x}{dt^2}=0, \quad \frac{d^2y}{dt^2}=-\frac{y}{a^2}, \quad \frac{d^2z}{dt^2}=-\frac{z}{a^2}.$$

From the first of these equations we have $x = kt + k'$, or, reckoning time from the instant when $x = 0$, and making the constant x-component of velocity unity,

$$x = t.$$

Hence, putting x for t in the other two equations, we have

$$\frac{d^2y}{dx^2}=-\frac{y}{a^2}, \quad \frac{d^2z}{dx^2}=-\frac{z}{a^2},$$

which are identical with the equations to the ray.

In like manner, the path of a ray under the conditions treated in Section II. of the previous paper is the same as that of a particle attracted towards the plane of reference by a force varying directly as the distance from this plane.

The differential equations of the motion of the particle are not (like those of the ray) limited to paths of small inclination.

As regards velocity, that of the particle increases and that of the ray diminishes as the line or plane of reference is approached.

Thus far most of our reasoning respecting rays has been merely approximate, and applicable only to rays of small inclination to the surfaces-of-equal-index. We now proceed to some rigorous deductions from the law of ray-curvature.

XIII. Let the surfaces-of-equal-index be concentric spheres. Required the law of index-variation which will cause all circles described about the common centre to be paths of rays.

Putting r for distance from centre, the required condition is

$$\frac{1}{r}=\frac{1}{\rho}=-\frac{d\log\mu}{dr};$$

that is,

$$d\log r + d\log\mu = 0,$$

or

$$\mu r = \mathrm{C}, \text{ an arbitrary constant.}$$

As μ cannot be less than unity, the law can only hold as far as $r=\mathrm{C}$. Within this distance, a ray, starting at any point, in a direction normal to the radius vector drawn to it from the centre will describe a circle about the centre. A ray starting at any other angle will describe an equiangular spiral having the centre for pole.

In any medium in which the surfaces-of-equal-index are concentric spheres, if we denote by θ the angle at which a ray cuts the surface-of-equal-index at the point considered, and by p the perpendicular from the centre on the tangent to the ray at this point, we have

$$\frac{1}{\rho}=\frac{1}{r}\frac{dp}{dr}, \quad \cos\theta=\frac{p}{r}.$$

Hence, by the law of ray-curvature, we have

$$\frac{1}{r}\frac{dp}{dr}=-\frac{d\log\mu}{dr}\cos\theta=-\frac{1}{\mu}\frac{d\mu}{dr}\frac{p}{r},$$

or

$$\frac{dp}{p}=-\frac{d\mu}{\mu},$$

or

$$\mu p=\mathrm{C},$$

a well-known result, usually obtained by regarding continuous variation of index as the limit of an indefinite number of small changes *per saltum.*

XIV. When the surfaces-of-equal-index are coaxial circular cylinders, the radius of any cylinder being called r, the value of $\frac{d\log\mu}{dr}$ is the product of $\frac{1}{\rho}$ by the secant of the angle at which a ray cuts the surface-of-equal-index at the point considered; and this product will be the same for all rays at the same value of r. If the ray be a helix described on one of the cylinders, the angle in question is zero, and the curvature $\frac{1}{\rho}$ of this helix will be equal to the value of $\frac{d\log\mu}{dr}$ for this cylinder.

Let the equations of the helix be

$$y=r\cos\frac{x}{a}, \quad z=r\sin\frac{x}{a}.$$

Then the value of $\frac{1}{\rho}$ is found to be $\frac{r}{a^2+r^2}$. Hence we have

$$\frac{d\log\mu}{dr}=-\frac{r}{a^2+r^2};$$

that is,

$$d \log \mu = -\tfrac{1}{2} d \log (a^2 + r^2),$$

or

$$\mu \sqrt{a^2 + r^2} = C.$$

In a medium in which this law prevails, every helix of step $2\pi a$, coaxial with the surfaces-of-equal-index, will be a ray-path; and, conversely, every helical ray in the medium will have the step $2\pi a$.

In the immediate vicinity of the axis, $\dfrac{r}{a^2 + r^2}$ is sensibly equal

to $\dfrac{r}{a^2}$, and the law of index now under consideration becomes

equivalent to that discussed in Section XI.

The step will be zero, and the helices will become circles, if a be zero, in which case the law of index becomes $\mu r = C$, as in Section XIII.

In general, when the surfaces of equal index are circular cylinders having for their common axis a line of maximum index, helical ray-paths will exist at all distances from the axis, and to each distance there will correspond a different step. The step for any distance r will in fact be determined by putting

$$\frac{d \log \mu}{dr} = - \frac{r}{a^2 + r^2},$$

and multiplying the value of a thus found by 2π.

XV. We found in Section II. of the previous paper that, for rays of small inclination to converge to foci in a medium in which μ is a function of the distance y from a plane of reference, the law of index-variation must be

$$\frac{d \log \mu}{dy} = - \frac{y}{a^2},$$

and that the rays will be curves of sines. Let us now examine the consequences of supposing all rays (whatever their inclinations) to be curves of sines, the surfaces-of-equal-index being still supposed to be parallel planes.

The curvature $\dfrac{1}{\rho}$ is, by the general law of ray-curvature, equal

to $- \dfrac{d \log \mu}{dy} \dfrac{dx}{ds}$, and is also equal to $- \dfrac{d^2 y}{dx^2} \left(\dfrac{dx}{ds}\right)^3$ by geometry. Hence we have

$$\frac{d \log \mu}{dy} = - \frac{1}{\rho} \frac{ds}{dx} = \frac{\dfrac{d^2 y}{dx^2}}{1 + \left(\dfrac{dy}{dx}\right)^2}. \quad . \quad . \quad . \quad (L)$$

But the equation of any ray through the origin of coordinates is to be of the form

$$y = b \sin \frac{x}{a};$$

whence

$$\frac{dy}{dx} = \frac{b}{a} \cos \frac{x}{a}, \quad \frac{d^2y}{dx^2} = -\frac{b}{a^2} \sin \frac{x}{a}.$$

Equation (L) therefore gives

$$\frac{d \log \mu}{dy} = \frac{-b \sin \dfrac{x}{a}}{a^2 + b^2 \cos^2 \dfrac{x}{a}} = \frac{-y}{a^2 + b^2 - y^2}.$$

But $\dfrac{d \log \mu}{dy}$ is to be simply a function of y, and therefore is not to vary from one ray to another. Hence the expression $a^2 + b^2$ in the denominator of the expression last found must be constant, and may be denoted by c^2. The equation may then be written

$$d \log \mu = \tfrac{1}{2} d \log (c^2 - y^2),$$

or

$$\mu = \frac{\mu_0}{c} \sqrt{c^2 - y^2},$$

μ_0 denoting the value of μ in the plane of reference. Hence it is clear that when μ varies as $\sqrt{c^2 - y^2}$, all rays will be curves of sines; and if b denote the amplitude of one of these curves, its half-wave-length will be

$$\pi a = \pi \sqrt{c^2 - b^2},$$

which diminishes as amplitude increases.

For small amplitudes we have

$$a = \sqrt{c^2 - b^2} = c \sqrt{1 - \frac{b^2}{c^2}} = c \left(1 - \frac{b^2}{2c^2}\right) \text{ nearly.}$$

Hence the limiting value of half-wave-length, or, in other words, the geometrical focal length, is πc; and the aberration from this focal length is always negative, its value being $-\pi \dfrac{b^2}{2c}$.

When b (and therefore also y) is small compared with a, the value of $\dfrac{d \log \mu}{dy}$ above obtained becomes sensibly equal to $-\dfrac{y}{a^2}$, thus confirming our previous approximate results.

XVI. Next let us seek a law of index-variation (the surfaces-

of-equal-index being still parallel planes) which will cause all rays to be arcs of circles.

Let a and b be the coordinates of the centre of one of these circles, the ray being supposed to pass through the origin, and the radius of the circle being called c. Then the equation of the ray will be

$$x^2 - 2ax + y^2 - 2by = 0,$$

the value of $\frac{ds}{dx}$ will be $\frac{c}{y-b}$; and since ρ is c, we have by equation (L),

$$\frac{d \log \mu}{dy} = -\frac{1}{\rho} \frac{ds}{dx} = -\frac{1}{c} \frac{c}{y-b} = -\frac{1}{y-b};$$

that is,

$$d \log \mu = -d \log (y-b),$$

or

$$\mu (y-b) = \mathrm{C}.$$

$y - b$ clearly denotes distance from a fixed plane, parallel to the plane of reference, and passing through the centre of the circular ray considered. The result which we have obtained accordingly indicates that, if the value of μ on one side of the plane of reference vary inversely as distance from a fixed plane on the other side of and parallel to the plane of reference, all rays on the first-mentioned side will be arcs of circles, having their centres in the said fixed plane.

If the medium is symmetrical about the plane of reference, there will be two such planes of centres, and the complete course of a ray will be a wavy line consisting of equal circular arcs succeeding one another in reversed positions. The half-wave-length may have any value from zero to infinity, the expression for it being

$$2a = 2 \sqrt{c^2 - b^2},$$

where c varies from one ray to another, while b is constant.

When the two planes of centres merge into one (or, in other words, when b is zero), the arcs become semicircles, and a curious question arises as to the course of a ray after cutting the plane of reference at right angles. If a ray once become normal to the planes-of-equal-index, what is to make it swerve to one side more than to the other? The difficulty vanishes, or at least is indefinitely postponed, when we remember that the velocity of the ray, being inversely as μ, diminishes to zero in approaching the plane of reference, and infinite time will be required to reach this plane.

The results which we have established in the present section might have been deduced at once from the undulatory theory

without any application of analysis.   For if μ is inversely as the distance from a fixed plane, velocity is directly as this distance, and the velocities at all points in a plane wave-front are therefore directly as their distances from the intersection of the wavefront with the fixed plane.   Hence the wave-front will swing round the line of intersection like a door upon its hinges, and each point in the wave-front will describe a circular arc, which will be the path of a ray.

XVII. Instead of employing the differential equation (L), we might have employed its integral, which can be reduced to the form

$$\mu \cos \theta = a,$$

a being a quantity which is constant for any one ray, but which varies from one ray to another.   This result has been previously established in Section IV., and is in fact merely a statement of the law of sines as applied to a medium in which the surfaces-of-equal-index are parallel planes.

Again, since the curvature of a ray depends only on the variation of $\log \mu$, we shall have precisely the same paths with the law $\mu = f(y)$ as with the law $\mu \propto f(y)$, since $\dfrac{d \log \mu}{dy}$ will have the same value in both cases.   The only limitation to this remark depends upon the circumstance that μ cannot be less than unity. A small constant factor applied to $f(y)$ may therefore diminish the range through which the law $\mu \propto f(y)$ can hold.

To obtain a convenient formula for determining the path of rays when μ is given as a function of y, observe that

$$1 + \left(\frac{dy}{dx}\right)^2 = \sec^2 \theta = \frac{\mu^2}{(\mu \cos \theta)^2} = \frac{\overline{f(y)}\,|^2}{a^2}, \quad . \quad . \quad \text{(M)}$$

whence

$$\frac{dx}{dy} = \frac{a}{\sqrt{\{\overline{f(y)}\,|^2 - a^2\}}}, \quad . \quad . \quad . \quad . \quad \text{(N)}$$

which is the general differential equation to the paths of rays in the medium, a being a parameter which varies from ray to ray. The following are some of the cases in which the equation can be integrated.

(1) $\mu \propto y$, $\dfrac{dx}{dy} = \dfrac{a}{\sqrt{y^2 - a^2}}$, the differential equation of a catenary, the axis of x being the line which is commonly employed as axis of x in treating of the properties of the curve, and a being the distance of the vertex of the curve from this axis.

The cases $\mu \propto y \pm b$ and $\mu \propto b - y$ are reducible to this by substitution.

(2) $\mu \propto \sqrt{y}$, $\dfrac{dx}{dy} = \dfrac{a}{\sqrt{y-a^2}}$, the differential equation of a parabola, with the axis of x for directrix, and a^2 for distance of vertex from directrix.

The cases $\mu \propto \sqrt{y \pm b}$ and $\mu \propto \sqrt{b-y}$ are reducible to this.

(3) $\mu \propto \dfrac{1}{y}$, $\dfrac{dx}{dy} = \dfrac{a}{\sqrt{\dfrac{1}{y^2}-a^2}} = \dfrac{ay}{\sqrt{1-a^2 y^2}}$, the differential

equation to a circle of radius $\dfrac{1}{a}$ having its centre on the axis of x. This result agrees with Section XVI. The cases $\mu \propto \dfrac{1}{y \pm b}$ and $\mu \propto \dfrac{1}{b-y}$ are reducible to this.

(4) $\mu \propto \dfrac{1}{\sqrt{y}}$, $\dfrac{dx}{dy} = \dfrac{a}{\sqrt{\dfrac{1}{y}-a^2}} = \sqrt{\dfrac{y}{\dfrac{1}{a^2}-y}}$, the differen-

tial equation of a cycloid generated by a circle of diameter $\dfrac{1}{a^2}$ rolling along the axis of x. The cases $\mu \propto \dfrac{1}{\sqrt{y \pm b}}$ and $\mu \propto \dfrac{1}{\sqrt{b-y}}$ are reducible to this.

(5) $\mu \propto \sqrt{b^2-y^2}$, $\dfrac{dx}{dy} = \dfrac{a}{\sqrt{b^2-y^2-a^2}}$, $x+c = a \sin^{-1} \dfrac{y}{\sqrt{b^2-a^2}}$,

a result which agrees with Section XV. The case $\mu \propto \sqrt{q \pm py - y^2}$ is reducible to this.

(6) $\mu \propto \sqrt{y^2 \pm b^2}$, $\dfrac{dx}{dy} = \dfrac{a}{\sqrt{y^2 \pm b^2-a^2}}$, whence

$$2y = e^{\frac{x+c}{a}} + (a^2 \pm b^2) e^{-\frac{x+c}{a}},$$

or, determining c so as to make $\dfrac{dy}{dx}$ vanish with x,

$$2y = \sqrt{a^2 \pm b^2}(e^{\frac{x}{a}} + e^{-\frac{x}{a}}).$$

The ordinates of the curve have therefore the constant ratio $\dfrac{\sqrt{a^2 \pm b^2}}{a}$ to those of a catenary. The case $\mu \propto \sqrt{y^2 \pm py \pm q}$ is reducible to this.

(7) $\mu \propto e^{by}$, or $\dfrac{d \log \mu}{dy} = b$; so that the curvature for a given

inclination of ray is the same in all parts of the medium. We have

$$\frac{dx}{dy} = \frac{a}{\sqrt{(e^{2by} - a^2)}};$$

or, putting $e^{by} = z$,

$$\frac{dx}{dz} = \frac{a}{b} \frac{1}{z \sqrt{z^2 - a^2}}, \quad x + c = \frac{1}{b} \sec^{-1} \frac{z}{a}.$$

Making $c = 0$, this gives $e^{by} = a \sec bx$; and y will be infinite when $bx = \frac{\pi}{2}$, so that the line $x = \frac{\pi}{2b}$ is an asymptote.

Some, at least, of the foregoing seven cases are to be found in existing text-books and examination-papers; but I believe that the method here set forth is an improvement upon that generally adopted.

XVIII. The following additional proof of the general law of ray-curvature is interesting, as being directly deduced from the law of least time.

Let T denote the time of passage of a ray from a fixed point to the point whose coordinates are x, y, z. Let α, β, γ be the angles which the forward direction of the ray at x, y, z makes with the axes, and let μ be the index and v the velocity at x, y, z, so that $\mu = \frac{V}{v}$, where V denotes the velocity *in vacuo*.

Draw a short line from x, y, z in any direction perpendicular to the ray at this point, and let its direction-cosines be l, m, n. By the principle of least time (or, more correctly, of stationary time), the value of T will be the same (to the first order of small quantities) from the fixed point to both ends of this short line; that is,

$$l \frac{dT}{dx} + m \frac{dT}{dy} + n \frac{dT}{dz} = 0.$$

But also

$$l \cos \alpha + m \cos \beta + n \cos \gamma = 0 ;$$

therefore

$$\frac{\frac{dT}{dx}}{\cos \alpha} = \frac{\frac{dT}{dy}}{\cos \beta} = \frac{\frac{dT}{dz}}{\cos \gamma} = k \text{ suppose.}$$

Again, if we draw a short line along the ray from x, y, z, the values of T from the fixed point to the two ends of this line will differ by the time of traversing this line. Hence we have

$$\frac{dT}{dx} \cos \alpha + \frac{dT}{dy} \cos \beta + \frac{dT}{dz} \cos \gamma = \frac{1}{v}.$$

Substituting for $\frac{dT}{dx}$, $\frac{dT}{dy}$, $\frac{dT}{dz}$ their values $k\cos\alpha$, $k\cos\beta$, $k\cos\gamma$, we have

$$k = \frac{1}{v} = \frac{\mu}{V}.$$

Therefore

$$\cos\alpha = \frac{V}{\mu}\frac{dT}{dx}; \quad \cos\beta = \frac{V}{\mu}\frac{dT}{dy}; \quad \cos\gamma = \frac{V}{\mu}\frac{dT}{dz}.$$

To apply these last formulæ to the investigation of the curvature of the ray at any point, make the axis of x coincide with the tangent (drawn forwards), and the axis of y with the principal radius of curvature (drawn from the point towards the centre of curvature). Then, in the neighbourhood of the origin, we have, to the first order of small quantities,

$$\frac{V}{\mu}\frac{dT}{dy} = \cos\beta = \sin\alpha = \alpha ;$$

and the curvature is

$$\frac{1}{\rho} = \frac{d\alpha}{dx} = \frac{d}{dx}\left(\frac{V}{\mu}\frac{dT}{dy}\right) = -\frac{V}{\mu^2}\frac{d\mu}{dx}\frac{dT}{dy} + \frac{V}{\mu}\frac{d^2T}{dx\,dy}.$$

But

$$\frac{dT}{dy} = 0, \text{ and } \frac{dT}{dx} = \frac{1}{v} = \frac{\mu}{V}.$$

Therefore

$$\frac{1}{\rho} = \frac{V}{\mu}\frac{d^2T}{dx\,dy} = \frac{V}{\mu}\frac{d}{dy}\left(\frac{\mu}{V}\right) = \frac{1}{\mu}\frac{d\mu}{dy};$$

and, from our choice of axes, $\frac{d\mu}{dy}$ denotes the rate at which μ increases in travelling towards the centre of curvature.

Two other proofs will be found in Parkinson's 'Optics,' arts. 122, 123; the result there obtained, namely

$$\frac{\mu}{\rho} = \frac{d\mu}{dx}\frac{dy}{ds} - \frac{d\mu}{dy}\frac{dx}{ds},$$

being clearly equivalent to that which we have employed, since $\frac{dy}{ds}$ and $-\frac{dx}{ds}$ are the direction-cosines of ρ.

I may also remark that the formula

$$\frac{d\mu}{dx} = \frac{d}{ds}\left(\mu\frac{dx}{ds}\right),$$

which is usually obtained by a difficult application of the Calculus of Variations, can be immediately derived from the princi-

ples employed in this section.   For since

$$\frac{dx}{ds} = \frac{V}{\mu}\frac{dT}{dx}, \text{ and } \frac{dT}{ds} = \frac{\mu}{V},$$

we have

$$\frac{d\mu}{dx} = \frac{d}{dx}\left(V\frac{dT}{ds}\right) = \frac{d}{ds}\left(V\frac{dT}{dx}\right) = \frac{d}{ds}\left(\mu\frac{dx}{ds}\right).$$

For this proof I am indebted to Professor Clerk Maxwell.

XXXIII. *On a simple Device for projecting on a Screen the Deflections of the Needles of a Galvanometer.   By* ALFRED M. MAYER, *Ph.D., Professor of Physics in the Stevens Institute of Technology, Hoboken, New Jersey, U.S.A.*[*]

THE instrumental problem of obtaining on a screen the deflections of a galvanometer-needle in magnified proportions has occupied the thoughts of several physicists.   The subject is evidently one of considerable importance.   In delicate researches it is often necessary that the body of the observer should be removed from the instrument, while at the same time he must be able to observe the minute deflections of its needles.   In lectures before our college-classes many of the most interesting and fundamental phenomena of radiant heat, electricity, and magnetism are often either entirely omitted or imperfectly presented, in default of an instrument which can be constructed by any one at a small outlay of time and expense. The problem, therefore, has not been deemed below the serious attention of eminent investigators; and although there are some who consider such contrivances trivial, yet I imagine they would think otherwise if they had the habit of continued original investigation, or the proper ambition to address their students in the very language of Nature by bringing them face to face with those phenomena which form the sure foundations of our scientific reasoning.

   The method invented by Poggendorff, of observing the deflections of the galvanometer by reflecting to a screen a beam of light from a small mirror attached to the needles, has been used for many years.   Sir William Thomson and Professor Tyndall have extensively used this method; and it has the advantage of giving to the reflected beam an angular motion the double of that given to the mirror by the needles.   More recently Dr. Tyndall has devised an instrument on the principle of the megascope. He throws a vertical beam from an electric lamp on to the dial and needle of the galvanometer, and by means of a lens and

inclined mirror placed above them he obtains their images on the screen.

In the Number of this Journal for July, 1872, I published the description of a new form of "Lantern-Galvanometer," which article was subsequently republished in Dr. Carl's *Repertorium.* This instrument, although it served admirably in the experiments for which it was specially devised, yet is of difficult construction and of limited application when compared with the very simple apparatus I will now proceed to describe.

G is the glass shade of the galvanometer, on which, at *g*, are drawn in india-ink the vertical graduation-lines of the instrument. A is a piece of aluminium wire, to whose lower
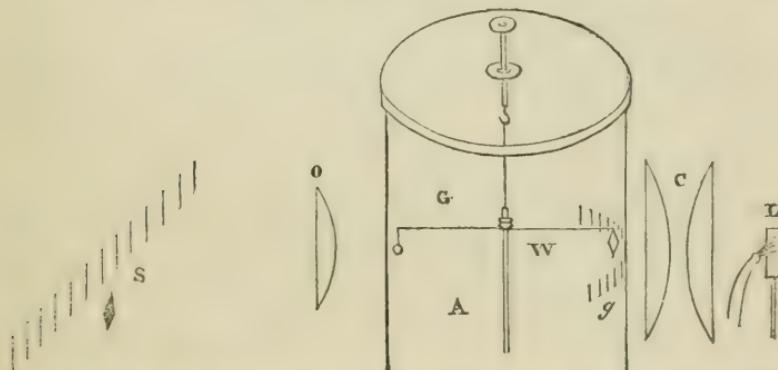
end are fixed the needles of the galvanometer, and whose upper end is perforated with a small hole, so that the system can be suspended by a silk fibre. A fine wire of German silver (W) is attached transversely to the aluminium wire, and has its ends bent downwards at right angles to its length. This transverse wire can be placed at any azimuth by rotating it around its centre, which is coiled two or three times round the vertical wire of aluminium. On one of the bent ends of the transverse wire is cemented a diamond-shaped piece of light paper or foil; and the other end carries a small ball of wax whose weight equals that of the piece of paper or foil. The diamond courses around the shade at about 1 millimetre from its interior surface, with its lower point just above the lines of graduation. At C are represented the condensing-lenses of an oxyhydrogen lantern whose jet and lime are at L. O is the objective, which gives on the screen the magnified image of pointer and scale, as seen at S. This scale is not graduated into equal angular divisions, but its units represent units of deflecting-force traversing the galvanometer; and this scale is therefore derived from a careful calibration of the instrument.

The sharpness of the image on the screen is admirable; and

with the calcium-light it is distinctly visible in a room considerably illuminated by daylight. With less illumination of the room I have used the instrument when the calcium-light was replaced by a kerosene-flame.

Evidently the precision of the indications of the apparatus just described are vitiated by the parallax of the index; for it does not describe a cylinder which is an extension of the one on which are drawn the graduations. This error is avoided by cementing on the inside of the shade a curved piece of glass whose radius of curvature equals the arm carrying the index, and whose centre coincides with the axis of the aluminium wire. With this modification in the apparatus I have succeeded in reading with precision deflections to 6′ of arc.

By the following arrangement, deflections to 1′ in an arc extending 5° on each side of the 0-point can be determined. A thin slip of microscope-cover glass is coated with a layer of black varnish, and through this varnish are cut, in a dividing-engine, fine equidistant lines. The diamond-shaped pointer is replaced by a light piece of cover-glass, also coated with varnish, and having cut on it one fine vertical line. These lines are illuminated by the lantern; and in front of them is placed an inch or an inch-and-a-half objective. On the screen we have the graduations as a series of bright lines on a dark ground, and along them moves the bright index-line of the pointer.

The zero-points of the scales can be brought accurately to coincide with the normal position of the index by revolving the shade on its base; and by turning the transverse wire so that it points towards the screen when the needles of the galvanometer have come to rest, we can readily project the image of the index and scale in any desired direction.

Although there are some advantages in having the scales attached to the galvanometer and in obtaining on the screen their magnified images, yet we can save much time in the construction of the apparatus by substituting for them scales drawn directly on the screen in very black india-ink.

My experience with this instrument has led me to prefer the use of only one magnetic needle, the one enclosed in the coil of the galvanometer; and this needle I render more or less astatic by means of a damping-magnet placed above the galvanometer and sliding on a vertical rod and rotating on its centre around the same. By means of the magnet, one can with expedition adapt the sensitiveness of the instrument to the requirements of special experiments; and thus the galvanometer is admirably suited for all experiments on radiant heat, electricity, or magneto-electricity. In fact, on holding my hand at a dis-

tance of 15 centimetres from the face of a thermopile without its conical reflector, I have often deflected the needle 30°.

One might suppose that the heat from the source of light would cause currents of air in the shade and make the needle fluctuate; but I have not met with this difficulty; and should it arise, it can be removed by placing in front of the condensing-lenses a glass tank containing an aqueous solution of alum.

The evident superiority of this instrument in simplicity of construction over all preceding devices will at once commend it; indeed any one in an hour or two can convert his galvanometer into a more convenient and precise instrument for research, and for the illustration of phenomena which have heretofore been presented to students only after considerable trouble on the part of the Professor. Now, however, the whole of the beautiful phenomena of radiant heat, which require a delicate instrument for their evolution, can be exhibited with ease and pleasure before our college-classes.

December 31, 1872.

XXXIV. *On Differential Galvanometers.*
By Louis Schwendler, *Esq.*[*]
Continued from vol. xliv. p. 170.

THE first part of this investigation concluded with the following question :—

What general condition must be fulfilled in the construction of any differential galvanometer in order to make a simultaneous maximum sensitiveness possible with respect to an alteration of external resistance in either of the differential branches?

To answer this question, it will be necessary to remember that the condition of a simultaneous maximum sensitiveness at or near balance was expressed by three equations, namely,

$$\frac{(w-g)(w'+g')+f(w+w'+g'-g)}{p(g-w)g'} = \frac{2(g+w+f)}{2\sqrt{g}\sqrt{g'}-p(g+w)}, \quad \text{(II.)}$$

$$\frac{(w'-g')(w+g)+f(w+w'+g-g')}{\frac{(g'-w')}{p} \cdot g} = \frac{2(g'+w'+f)}{2\sqrt{g}\sqrt{g'}-\frac{g'+w'}{p}}. \quad \text{(II'.)}$$

and

$$g'+w'-p\frac{\sqrt{g'}}{\sqrt{g}}(g+w)=0, \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \text{(I.)}$$

g and g' being the resistances of the two differential coils, w and w' the two resistances at which balance actually arrives, f

the total resistance in the battery branch, and p an absolute number expressing what was termed the "*mechanical arrangement*" of the differential galvanometer under consideration.

By these three equations, which are independent of each other, g, g', and p can be expressed in terms of w, w', and f.

By equation (I.) we have, at or very near balance,

$$p = \frac{g' + w'}{g + w} \cdot \frac{\sqrt{g}}{\sqrt{g'}},$$

which value, substituted in equations (II.) and (II'.), gives

$$\frac{(w - g)(w' + g') + f(w + w' + g' - g)}{(g' + w')(g - w)g'} = \frac{2(g + w + f)}{(g' - w')(g + w)} \quad \text{. (II.)}$$

and

$$\frac{(w' - g')(w + g) + f(w + w' + g - g')}{(g + w)(g' - w')g} = \frac{2(g' + w' + f)}{(g - w)(g' + w')}; \quad \text{. (II'.)}$$

and from these two equations g and g' may be developed.

This is best done by subtracting equation (II.) from equation (II'.), when, after reduction, we get

$$(w'g - wg')(w'g + wg' + gg' + ww')$$
$$= -f(g + g' + w + w')(w'g - wg'). \quad \text{. . (III.)}$$

Now it must be remembered that, with respect to our physical problem, f, w, w', g, and g' represent nothing else but electrical resistances, and that they have therefore to be taken in any formula as quantities of the same sign (say positive).

Consequently the above equation (III.) would contain a mathematical impossibility (a positive quantity equal to a negative quantity) whenever the common factor $w'g - wg'$ is different from zero.

In other words, equation (III.) can only be fulfilled if we always have

$$w'g - wg' = 0. \quad \text{. . . . . . (IV.)}$$

This simple relation between the resistances at which balance arrives and the resistances of the two differential coils expresses not only the necessary and sufficient condition under which a simultaneous maximum sensitiveness can exist, but it also affords an easy means of getting at once those special values of g, g', and p, which only solve the physical problem.

Substituting the value of either g or g', as given by equation (IV.), in equations (II.) and (II'.) and developing g and g', we have

$$*g = -\frac{1}{3}\left(w + f\frac{(w+w')}{2w'}\right) + \frac{2}{3}\sqrt{w^2 + \frac{w}{w'}(w+w')f + \frac{(w+w')^2}{16w'^2}f^2}, (a)$$

$$*g' = -\frac{1}{3}\left(w' + f\frac{(w+w')}{2w}\right) + \frac{2}{3}\sqrt{w'^2 + \frac{w'}{w}(w+w')f + \frac{(w+w')^2}{16w^2}f^2}, (b)$$

the negative signs of the square roots having been omitted, since they would obviously make g and g' negative, values which cannot solve the physical question.

Further, if we introduce the ratio $\frac{g'}{g} = \frac{w'}{w}$, given by equation (IV.), into equation (I.), and develop p, we get

$$p^2 = \frac{w'}{w}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (c)$$

This latter expression shows the very simple relation which must exist between the *mechanical arrangement* of *any* differential galvanometer and the two resistances at which balance is arrived at in order to make a simultaneous maximum sensitiveness possible.

Thus if the ratio of the two resistances at which balance arrives is fixed, the mechanical arrangement p cannot be chosen arbitrarily, but must be identical with this ratio. This is in fact the answer to the question put at the beginning of this paper.

However, the meaning of this result will be made even still clearer if we revert to equation (I.), by which we have

$$p\frac{\sqrt{g'}}{\sqrt{g}} = \frac{g'+w'}{g+w} = C, \quad . \quad . \quad . \quad (I.)$$

expressing the ratio between the total resistances in the two differential branches when balance is established, which ratio is generally known under the name *Constant of the Differential Galvanometer.*

Substituting in the above expression (I.) the value of $\frac{g'}{g} = \frac{w'}{w}$, from equation (IV.) we get at once

$$\frac{w'}{w} = C; \quad . \quad . \quad . \quad . \quad . \quad . \quad (d)$$

and as a second answer to the question put at the beginning of this paper we have therefore :—

A simultaneous maximum sensitiveness with respect to an alteration of external resistance in either branch of any differential galvanometer can be obtained only if the constant of the differential galvanometer is equal to the ratio of the two resistances at which balance arrives; and this clearly necessitates that the resistances of the respective coils to which w and w' belong should stand in the same ratio.

* See note at end.

The general problem may now be regarded as solved by the following four general expressions:—

$$g = -\frac{1}{3}\left(w + f\frac{(w+w')}{2w'}\right) + \frac{2}{3}\sqrt{w^2 + \frac{w}{w'}(w+w')f + \frac{(w+w')^2}{16w'^2}f^2}, \quad (a)$$

$$g' = \frac{w'}{w}g, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (b)$$

$$p^2 = \frac{w'}{w}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (c)$$

$$C = \frac{w'}{w}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (d)$$

Additional Remarks.

In the foregoing it has not been shown that the values g and g' expressed by equations (a) and (b) must necessarily correspond to a maximum sensitiveness of the differential galvanometer, because it was clear *à priori* that the function by which the deflection is expressed is of such a nature that no minimum with respect to g and g' is possible. However, to complete the solution mathematically, the following is a very short proof that the values of g and g' really do correspond to a maximum sensitiveness of the differential galvanometer under consideration.

Reverting to one of the expressions for the deflection $a°$ which any differential galvanometer gives before balance is arrived at, we had $a° \propto K\frac{\sqrt{g}}{N}\Delta$; and as the increase of deflection at or near balance is identical with the deflection itself, and, further, as the law which binds the resistance of the differential coils to the other resistances in the circuit in order to have a maximum sensitiveness is of practical interest only when the needle is at, or very nearly at, balance, we can solve the question at once by making $a°$ a maximum with respect to g and g', if we only suppose Δ constant and small enough; and as K is known to be independent of g and g', the deflection $a°$ will be a maximum if $\frac{\sqrt{g}}{N}$ is a maximum for any constant Δ (zero included).

Further, we know that $g' = Cg$, which value for g' in N substituted will make the latter a function of g only, and consequently $\frac{\sqrt{g}}{N}$ also. We have therefore to deal with a single maximum or minimum; and, according to well-known rules, we have

$$\frac{da}{dg} = \frac{N - 2g \dfrac{dN}{dg}}{2\sqrt{g}N^2} = \frac{U}{V}$$

and

$$\frac{d^2a}{dg^2} = \frac{V\dfrac{dU}{dg} - U\dfrac{dV}{dg}}{V^2};$$

but

$$\frac{da}{dg} = 0;$$

it follows that $U = 0$;

$$\therefore \frac{d^2a}{dg^2} = \frac{1}{V}\frac{dU}{dg}.$$

Now

$$\frac{dU}{dg} = -\left(\frac{dN}{dg} + 2g\frac{d^2N}{dg^2}\right);$$

but $\dfrac{dN}{dg}$, as well as $\dfrac{d^2N}{dg^2}$, being invariably positive, it follows that $\dfrac{dU}{dg}$ is invariably negative; and as, further, V is always positive, it follows finally that $\dfrac{d^2a}{dg^2}$ is always negative, or the value of g obtained by equation $\dfrac{da}{dg} = 0$ corresponds to a maximum sensitiveness of the differential galvanometer.

In a similar way it can be shown that the value of g' obtained by equation $\dfrac{da}{dg'} = 0$ corresponds also to a maximum sensitiveness of the differential galvanometer.

This is in fact a second and far more simple solution of the problem. However, it is by no means as general, nor does it adhere as closely to the spirit of analysis, as the first more complicated solution.

Effect of Shunts.—It is clear that the introduction of shunts cannot alter the general results as given in equations (*a*), (*b*), (*c*), and (*d*), as long as the shunts are used merely for the purpose of carrying off a fixed quantity of current without in themselves having any direct magnetic action on the needle.

However, to avoid misunderstanding, it is well to remember that, in the case of shunts being used, the values to be given to w and w' in the above equations are *not* those at which balance actually arrives, but those at which balance would arrive if no shunts were used; *i. e.* the resistance at which balance is esta-

blished when using shunts must be multiplied by the multiplying-power of their respective shunts before they are to be substituted in the equations (a), (b), (c), and (d).

Mechanical Arrangement designed by p.—The condition which must be fulfilled in the construction of any differential galvanometer to make a simultaneous maximum sensitiveness possible was expressed by

$$p^2 = \frac{w'}{w}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (c)$$

while $p = \dfrac{m'n'}{mn}$; and it will be now instructive to inquire what special physical meaning equation (c) has.

By m was understood the magnetic effect of an average convolution (*i. e.* one of average size and mean distance from the magnet acted upon when the latter is parallel to the plane of the convolutions) in the differential coil of resistance g when a current of unit strength passes through it. Similarly m' was the magnetic effect of an average convolution in the other differential coil of resistance g'.

Further, n and n' were quantities expressed by

$$U = n \sqrt{g},$$

and

$$U' = n' \sqrt{g'},$$

U and U′ being the number of convolutions in the two coils g and g' respectively.

Now we will call A half the cross section of the coil g (cut through the coil normal to the direction of the convolutions), which section, as the wire is to be supposed uniformly coiled, must be uniform throughout.

Thus we have generally

$$\frac{A}{c(q + \delta)} = U$$

wherever the normal cut through the coil is taken.

c is a constant indicating the manner of coiling, either by dividing the cross section A into squares, hexagons, or in any other way, but always supposing that, however the coiling of the wire may have been done, it has been done uniformly throughout the coil. (This supposition is quite sufficiently nearly fulfilled in practice, because the coiling should always be executed with the greatest possible care; and, further, the wire can be supposed practically of equal thickness throughout the coil.)

q is the metallic section of the wire, and δ the non-metallic section due to the necessary insulating covering of the wire.

Further, we have $g = U \dfrac{b}{q\lambda}$, where b is the length of an average convolution, and λ the absolute conductivity of the wire material, supposed to be a constant for the coil.

Now, for brevity's sake, we will suppose that δ, the cross section of the insulating covering, can be neglected against q the metallic cross section of the wire.

Consequently we have

$$\frac{A}{cq} = U \text{ (approximately)}$$

and

$$g = U \frac{b}{q\lambda} ;$$

$$\therefore \; U = \sqrt{\frac{A\lambda}{bc}} \cdot \sqrt{g},$$

or

$$n = \sqrt{\frac{A\lambda}{bc}} ;$$

similarly,

$$n' = \sqrt{\frac{A'\lambda'}{b'c'}} ;$$

$$\therefore \; \frac{n'}{n} = \sqrt{\frac{A'\lambda'bc}{A\lambda b'c'}}.$$

But using wire of the same conductivity in both the differential coils, which should be as high as is possible to procure it, and further supposing the manner of coiling to be identical in both coils, we have

$$\lambda = \lambda',$$
$$c = c' ;$$
$$\therefore \; \frac{n'}{n} = \sqrt{\frac{A'}{A} \cdot \frac{b}{b'}}.$$

Further, we know that if the shape and dimensions of each coil are given, and in addition also their distance from the magnet acted upon, it will always be possible to calculate m and m', though it may often present mathematical difficulties, especially if the forms of the two coils differ from each other and are also not circular. This latter condition is generally necessitated in order to obtain the greatest absolute magnetic action of each coil in as small a space as possible.

However, it is clear that we may assume generally that the two coils have each an average convolution of identical shape and of the same length, placed at an equal distance from the

magnet acted upon, and that therefore the magnetic action of each coil is dependent on the number of convolutions only.

In this case we have evidently

$$m = m',$$
$$b = b',$$
$$\frac{n'}{n} = \sqrt{\frac{A'}{A}};$$

and as

$$p = \frac{n'}{n} \cdot \frac{m'}{m},$$

we have, finally,

$$\frac{A'}{A} = \frac{w'}{w}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (e)$$

Equation (e) shows at once that under the supposed conditions (*i. e.* when the average convolutions in each coil are of equal size and shape) the wire used in either coil is of the same absolute conductivity, and that the thickness of the insulating material can be neglected against the diameter of the wire :—

The wire used for filling each coil must be invariably of the same diameter ; otherwise a maximum sensitiveness is impossible.

How the above simple law expressed by equation (e) would be altered when the given suppositions were not fulfilled must be found by further calculation ; but as the latter is intricate and a more general result is not required in practice, I shall dispense at present with this labour.

Special Differential Galvanometers.—Here shall be given the special expressions to which the general equations (a), (b), (c), and (d) are reduced when certain conditions are presupposed.

1st case.—When w and w', the two resistances at which balance is arrived at, are so large that f, the resistance of the testing battery, can be neglected against either of them without perceptible error. Substituting therefore $f = 0$ in equations (a) and (b), we get

$$g = \frac{w}{3}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (a)$$

$$g = \frac{w'}{3}; \quad . \quad . \quad . \quad . \quad . \quad . \quad (b)$$

and the other two remain as they are, namely

$$p^2 = \frac{w'}{w}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (c)$$

$$C = \frac{w'}{w}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (d)$$

2nd case.—When the battery resistance f cannot be neglected against either w or w', but when the two resistances at which balance is arrived at are invariably equal.

Thus, substituting in the general equation

$$w = w' = w,$$

we get

$$g = g' = g = -\frac{w+f}{3} + \frac{1}{3}\sqrt{4w^2 + 8fw + f^2}, \quad . \quad . \quad (a, b)$$

$$p^2 = 1, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (c)$$

$$C = 1. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (d)$$

3rd case.—When the conditions given under 1 and 2 are both fulfilled, or

$$w = w' = w,$$

and

$$f = 0;$$

then we have

$$g = g' = g = \frac{w}{3}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (a, b)$$

$$p^2 = 1, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (c)$$

$$C = 1, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (d)$$

the very same result which was obtained by direct reasoning at the beginning of this paper.

Applications.—Though the problem in its generality has now been entirely solved, it will not perhaps be considered irrelevant to add here some applications.

For our purpose differential galvanometers may be conveniently divided into two classes, viz. those in which the resistances to be measured vary within narrow limits, and those where these limits are extremely wide.

To the first class belong the differential galvanometers which are used for indicating temperature by the variation of the resistance of a metallic wire exposed to the temperature to be measured—as, for instance, C. W. Siemens's resistance thermometer for measuring comparatively low temperatures, or his electric pyrometer for measuring the high temperature in furnaces.

It is clear that for such instruments the law of maximum sensitiveness should best be fulfilled for the average resistance to be measured, which average resistance under given circumstances is always known.

To the second class belong those differential galvanometers which are used for testing telegraph-lines, at present the most important application of these instruments. In this case each

differential coil should consist of separate coils connected with a commutator in such a manner that it is convenient to alter the resistance of each coil according to circumstances, *i. e.* connecting all the separate coils in each differential coil parallel when the resistances to be measured are comparatively low, and all the separate coils consecutively if the resistances to be measured are high &c., fulfilling in each case the law of maximum sensitiveness for certain resistances, which are to be determined under different circumstances differently, but always bearing in mind that it is more desirable to fulfil the law of maximum sensitiveness for high resistances (when the testing current in itself is obviously weak) than for low resistances.

An example will show this clearer. Say, for instance, a differential galvanometer has to be constructed for measuring resistances between 1 and 10,000. A Siemens's comparison box of the usual kind $\left(\dfrac{1}{10,000}\right)$ being at disposal, it will be convenient and practical to decide that the two differential coils should be of equal magnetic momentum; from which it follows that C as well as p must be unity, or, in other words, that the two coils must be of equal size, shape, and distance from the needle, and must also have equal resistances, *i. e.* must be filled with copper wire of the same diameter. The resistance of each coil is then found by

$$g = -\frac{w+f}{3} + \frac{1}{3}\sqrt{4w^2 + 8fw + f^2},$$

where f is the resistance of the battery, and w a certain value between 1 and 10,000, the two limits of measurement. The question now remains to determine w.

It is clear that the law of maximum sensitiveness has not to be fulfilled for either limit, because they represent only one of the 10,000 different resistances which have to be measured; but it is also clear that to fulfil the law for the average of the two given limits would be equally wrong, inasmuch as the maximum sensitiveness is far more required towards the highest than the lowest limit. We may assume, therefore, that it is desirable to fulfil the law for the average of the average and the highest limit, which gives

$$w = 7500,$$

against which the resistance of the battery may always be neglected.

Consequently we have

$$g = \frac{w}{3} = 2500$$

for each coil.

Now, if the coil be small and consequently the wire to be used for filling it is thin, the value $g = 2500$ wants a correction to make allowance for the thickness of the insulating material, by which g becomes somewhat smaller[*].

Before concluding, I may remark that the question of the best resistance of the coil, when the resistance to be measured varies between two fixed or variable limits, can be solved mathematically by the application of the variation calculus.

XXXV. *On Fractional Distillation.*
By J. C. Glashan, *Strathroy, Ontario*[†].

IN the current Number of the Philosophical Magazine Mr. J. A. Wanklyn states that up to the present there is no theory of fractional distillation, and thereupon proceeds to deduce one. The following I believe to be the mathematical representation of the law of the rate of separation of the liquids, and indirectly involving the theory of the process. The evolution of this theory is an inverse operation, giving a result involving "an arbitrary function;" but it may be noted in passing that Mr. Wanklyn's theory satisfies the equations—that, in fact, they are very easily deduced by it. If the experiments are correctly made and the observations of p and q, formula (XI.) as calculated directly from (VII.) and indirectly through (X.) affords a test of the truth of the mathematical theory; or, to test the truth of the theory, calculate r from examination of two portions of a continuously formed distillate.

Let $a + b$ represent a homogeneous mixture of a units of a

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[*] These expressions for g and g' must be corrected if the thickness of the insulating covering of the wire cannot be neglected against its diameter. The formula by which this correction can be made was given by me in the Philosophical Magazine, January 1867—namely,

$$\text{corrected } g = cg(1 - \sqrt[4]{gm^2}),$$

where $g =$ the resistance to be corrected and expressed in Siemens's units, and

$$m = \delta^{\iota} \sqrt{\frac{c\pi\lambda}{AB}}.$$

$\delta =$ radial thickness of the insulating covering expressed in millimetres.

$c =$ a coefficient expressing the arrangement adopted for filling the available space uniformly with wire; namely, if we suppose that the cross section of the coil, by filling it up with wire, is divided into squares, we have $c = 4$; if in hexagons, $c = 3.4$. &c.

$\lambda =$ absolute conductivity of the wire-material (IIg = 1 at freezing-point).

$A =$ half the section of the coil in question when cut normal to the direction of the convolutions, and always expressed in square millimetres.

$B =$ length of an average convolution in the coil, and expressed in metres.

† Communicated by the Author.

liquid A of volatility w, and b units of a liquid B of volatility v. Distil till there remains $a_t + b_t$, giving a distillate $(a - a_t) + (b - b_t)$; then $b = (a_t : a)^{v : w}$; or putting p for $a_t : a$, and r for $v : w$, we have the very simple formulæ:

$$\text{1st still-liquor,} \quad a + b, \quad \dots \dots \dots \quad \text{(I.)}$$
$$\text{2nd ,, ,,} \quad ap + bp^r, \quad \dots \dots \dots \quad \text{(II.)}$$
$$\text{Distillate,} \quad a(1-p) + b(1-p^r), \quad \dots \quad \text{(III.)}$$
$$(\text{Distillate})^n, \quad a(1-p)^n + b(1-p^r)^n. \quad \dots \quad \text{(IV.)}$$

(Distillate)^n means that it is the distillate of (distillate)$^{n-1}$.
Solutions of (III.) and (IV.) of 1 degree of approximation are

$$(a + br)(1-p), \quad \dots \dots \dots \quad \text{(V.)}$$
$$(a + br^n)(1-p)^n. \quad \dots \dots \dots \quad \text{(VI.)}$$

As stated above, these formulæ may be obtained by Mr. Wanklyn's theory. Thus, ratio of liquids in original liquor,

$$a : b ;$$

in first infinitesimal distillate,

$$da : br\frac{da}{a} ;$$

in remaining liquor,

$$a\left(1 - \frac{da}{a}\right) : b\left(1 - r\frac{da}{a}\right).$$

Continue distillation to the separation of n distillates.   Ratio of remaining liquor,

$$a\left(1 - \frac{da}{a}\right)^n : b\left(1 - r\frac{da}{a}\right)^n.$$

This for a finite distillation, or n infinitely large, becomes

$$a\left(1 - \frac{da}{a}\right)^n : b\left\{\left(1 - \frac{da}{a}\right)^n\right\}^r = ap \div bp^r ;$$

for limit $\left(1 - r\dfrac{da}{a}\right)^n = $ limit $\left(1 - \dfrac{da}{a}\right)^{rn}$, and, by definition of p,

$$\left(1 - \frac{da}{a}\right)^n = p.$$

Mr. Wanklyn uses (V.) and obtains $r = 9 \cdot 6$ for ammonia (water $= 1$); but from consideration of the composition of the remaining (or rather last distilling) liquor he concludes $r > 13 < 14$. The correct value as found from (III.) is

$$\frac{2 - 1 \cdot 52}{2 - 1 \cdot 95} = 12 \cdot 7483.$$

Formula (V.) shows distinctly that for p^r nearly $= 1$ the strength of the distillate is approximately proportional to that of the mother-liquor for the same fraction of A distilled (observed by Mr. Wanklyn). If $r > 1$ or B volatile compared with A, formula (IV.) shows that, with repeated distillations of the early-passing-over distillate, the B strength of the distillate will rapidly increase with the number of distillations, and formula (VI.) shows that, to a rude approximation, the rate of increase is in geometrical progression.

It has been assumed that the distillation is pure, *i. e.* that the vapour does not carry over any liquor held in mechanical suspension ; for in this case the coadhesive function of the liquids will influence the result. Further, it is supposed that the whole distillate has been separated from the still-liquor and, if it is used, collected. This requires that no part of the distillate (especially at the beginning) condenses on the sides of the still, in time returning to the still-liquor. The following formulæ afford a test of the accuracy of the experiment (assuming the theory to be true), and also a means of correctly calculating r, even with a common still properly protected, although the second of the above-mentioned difficulties tends to vitiate early-made observations or those continuous from the beginning.

1st still-liquor, $\quad a + b,$ . . . . (VII.)

2nd ,, ,, $\quad ap + bp^r,$ . . . . (VIII.)

3rd ,, ,, $\quad (ap)q + (bp^r)q^r.$ . . . . (IX.)

1st distillate from (VII.),
$$a(1-p) + b(1-p^r), \quad . \quad . \quad . \quad . \quad . \quad (X.)$$

1st distillate from (VIII.),
$$ap(1-q) + bp^r(1-q^r). \quad . \quad . \quad . \quad . \quad (XI.)$$

Let r be calculated from (X.) and (VII.), (VIII.) is determined by observation for (X.) ; thus r may also be calculated from (XI.) and (VIII.). The latter value of r will be the more reliable if the elements of (X.) be accurately observed. The distillation is supposed to be continuous. The value of r may also be calculated very accurately from (II.), obtained from (I.) by evaporation under the air-pump.

For simplicity of formulæ I have assumed the mixture to be homogeneous and to remain so throughout the operation ; but the instant evaporation commences this condition is violated. Thus in the deduction of the formulæ through Mr. Wanklyn's theory, the process is true only for the first infinitesimal distillation, and all subsequent distillations will involve a function of

a quantity which represents the energy of restoration of the homogeneous condition. Again, since the mixture is not homogeneous in respect of its temperature, the restorative function will be affected by the velocity of convection. It is easy to show that formula (II.) is true for a non-homogeneous mixture if such mixture is a perfect liquid. This view shows that (II.) applies to liquids approaching the *perfect* condition, but does not apply to syrupy liquids, to thick oils, and such like. The differential formulæ for these show a tendency to thickening at the top, and also at the point of application of the heat. This tendency increases with the viscosity, and after a certain point may become cumulative, the energy of convection and heat-conducting power of the mixture as it approaches the solid state determining where the cumulative effect will first begin. Attention must also be paid to the direction of the *heat-convection*, i. e. the direction in which the warmer currents flow. Thus, if the convection of water at a temperature above that of its point of maximum density be reckoned positive, its convection below the temperature of the said point must be taken as negative. The deduction of the differential form of (II.) for a non-perfect liquid is not the object of this paper, which was commenced merely to give a simple means of correctly calculating r from Mr. Wanklyn's theory; but I may state that the correct formulæ show that a *crust* may be formed in two ways—(1) by negative convection, (2) by increase of viscosity accompanied by very slight variation of temperature in the parts of the evaporating liquid, or by very slow evaporation, or by a very great difference of temperature between the liquid and the "space" immediately beyond it.

February 22, 1873.

Note.—On examining the theory by which I was led to formula (II.), I find that if the energy of return to homogeneity remains sensibly uniform for all mixtures from $a + b$ to $ar + b$, the proposed formula holds, provided the mixture does not pass during the operation through a point of maximum or minimum density.

February 24, 1873.

XXXVI. *On the Action of Solid Bodies on Gaseous Supersaturated Solutions.* By CHARLES TOMLINSON, *F.R.S.*[*]

IN the March Number of the Philosophical Magazine I gave a translation of Dr. Henrici's paper on the above subject from Poggendorff's *Annalen* for December last. Having also

worked in the same field and published the results of my labours in this Magazine*, it may not be thought out of place if I here offer some opinions on Dr. Henrici's results, which stand out, as he admits, in singular contrast to my own.

In an inquiry of this kind it is, of course, necessary to define accurately what is meant by (1) a supersaturated gaseous solution, (2) a chemically clean surface in contradistinction to (3) one that is not clean. I do not object to Dr. Henrici's statement as to the first; but he seems to me to have but an imperfect idea as to the second. Having succeeded in making metallic surfaces sufficiently clean for voltaic contact by rubbing them with finely powdered pumice-stone spread on leather, he adopts this same process for making clean the glass rods, metallic wires, and some of the other bodies used in his inquiry; and he objects to one of the methods used by me for the purpose, namely heating them in the flame of a spirit-lamp, on the ground that the heat, instead of getting rid of the impure film that covers them more or less, simply converts that film into a porous body which still invests the surface.

The method of cleaning the wires &c. by means of pumice-powder spread on leather is absolutely of no value with reference to the present inquiry. The surfaces thus treated are still unclean, and naturally become covered with bubbles on immersing them in soda-water &c. So also in the application of heat, Dr. Henrici made use of a small alcohol-flame, and passed the bodies over and above it rather than through it; whereas he should have made his platinum wires red-hot and even white-hot.

In several of my published papers† I have endeavoured to show that the obscure and often contradictory behaviour of solids as nuclei, in separating gas, or vapour, or salt from their supersaturated solutions, becomes clear by considering whether the solids used as nuclei, or the walls of the flasks, test-glasses, and other containing vessels were or were not chemically clean as to surface at the moment of contact with the solution.

A body was defined as chemically clean the surface of which is entirely free from any substance foreign to its own composition. And it will be remarked that I speak of *surface* only. A glass rod is chemically clean, although a particle of carbon, or of oxide of iron, or other matter be enclosed and shut up within it; but not so if that particle reach and form a portion of the surface itself. So also a stick of tallow, stearine, paraffin, resin, sulphur, &c. is chemically clean so long as its surface falls under the definition just given.

* Phil. Mag. for August and September 1867.
† Some of the definitions given in the text are from the Philosophical Transactions for 1871, p. 51.

Catharization is the act of cleaning the surface of bodies from all alien matter; and the substance is said to be catharized when its surface is so cleared.

The methods of doing this are various. The action of flame or of strong sulphuric or nitric acid, or of alkaline solutions is efficacious according to the nature of the surface. In some cases steeping the solids in water during several days may suffice; in others boiling them in acid or alkaline solutions, or rubbing them between corks or platinum-foil while immersed in the strongest commercial oil of vitriol, or washing them with alcohol or ether may be necessary, but always finishing with a copious rinsing in a stream of water. Should any one of these methods fail in any particular instance, another may be resorted to. For example, a short cylinder of phosphorus cut out of a stick that had been previously scraped was attached loopwise to a platinum wire, and so lowered into a test-tube (cleaned by the action of strong sulphuric acid and copious rinsing) containing soda-water[*]. Not a single bubble of gas appeared on the walls of the tube; but the phosphorus and the wire were abundantly covered. These were lowered into a tube containing spirits of wine, and after some minutes taken out and rinsed with clean water and again lowered into the soda-water; but they were still active, nearly as much so as before. The wire and the phosphorus were then placed in washed ether, and on being returned to the soda-water the wire was perfectly inactive, but the phosphorus was as briskly active as before,—the fact being that chemically clean platinum wire is not nuclear, but chemically clean phosphorus is.

As every thing exposed to the air of a room or to the touch takes more or less a deposit or film of foreign matter, substances may be conveniently classed as *catharized* or *uncatharized* according as they have been or not so freed from foreign matter.

And it is perhaps not taking too much license with language to extend the term *catharized* (denoting, as it does, the condition of pure surface) to those substances whose surface has not required the process. Thus a flint stone in the rough has an uncatharized surface, and when immersed in soda-water becomes instantly covered with bubbles; but split it, and the inner surface of the pieces will for a time be clean, and if put into soda-water will not have a single bubble of gas upon it.

On the other hand, some forms of matter, although perfectly clean, become covered with bubbles the moment they are immersed in a supersaturated gaseous solution. Thus, newly formed fragments of a lump of resin, or of a roll of sulphur, or of a block

of stearine or paraffin, although chemically clean, become covered with bubbles as soon as they are placed in soda-water. But here other conditions are introduced, such as porosity, and the very different adhesion between resin &c. and air or gas, and resin &c. and water.

Dr. Henrici's idea of clean surfaces is such that, if previously cleaned according to his method with leather and pumice and exposed to the air, wiping them with a cloth or rinsing them is sufficient to restore their purity. A platinum wire or glass rod so cleaned and immersed in soda-water becomes covered with gas-bubbles; and this he urges as a proof of the chemical purity of such surfaces, and he even maintains that this is their proper function; whereas the very reverse is the truth.

Consider the problem in hand*. A supersaturated solution of a gas with its upper surface exposed to the air is always giving off gas either with effervescence or silently. It does so because the excess of gas has but a slight adhesion to the liquid, and the air is virtually a vacuum for it. Now the remaining surface of the liquid, or that confined by the sides of the vessel, may be regarded as being in exactly the same condition, subject, however, to two modifications—(1) the state of chemical purity of their surface, and (2) the pressure exerted by them virtually on the liquid. (1) Suppose the vessel to be of glass, and to be chemically clean according to the definition given above. No gas will be disengaged and no bubbles will form on the sides, because the adhesion between the sides and the gaseous solution is perfect; and therefore the sides may be regarded, *pro ratâ*, as merely a continuation of the liquid itself, and no bubbles will form there any more than in the central parts of the liquid. (2) But suppose the sides to be not chemically clean—to be dirty in fact; adhesion is diminished or destroyed, and therefore the surface of the liquid next to such sides is virtually as free as its upper surface: bubbles will consequently form here just as they do on the upper surface; but in the latter case they do not appear as bubbles (except during effervescence) because there is no pressure; the sides do exert pressure, and therefore bubbles are formed. It does not matter whether there be air or not between the sides and the liquid. It is no function of air to induce the liberation of gas or the formation of gas-bubbles. It is really want of adhesion. Now apply this to the case of a catharized glass rod, platinum wire, or a newly fractured surface of flint. Any one of these placed in the liquid does nothing more than form new sides, as it were, to the vessel; and its effect is merely that of the sides. If chemically clean, the glass rod &c. will form no bubbles round it; and hence it is inactive because its

* See Phil. Mag. Sept. 1867.

adhesion is perfect. If dirty, the surface of liquid in contact with it will be as free, or almost so, as the upper surface. We hope to give a more definite idea of the word " dirty " presently.

If further proof were wanting that Dr. Henrici has fallen into error in consequence of an inadequate conception of the term " chemically clean," it is to be found in his experiments and remarks on the action of mercury in liberating gas from solution. When what he terms " pure " mercury was put into soda-water, it " was immediately covered with rapidly swelling bubbles, which ascended, while others formed in their place. Or when by sha-king the glass the bubbles escaped from the mercury, new ones immediately covered it; and this result, notwithstanding the small quantity of gas in solution, could be repeated many times with scarcely any diminution. Even in but slightly impregnated water, in which other surfaces did not act, mercury, by separating numerous bubbles, displayed its surpassing activity. Pure mer-cury forms indeed the most perfect surface that can be used in these experiments, since it is perfectly wetted by water."

Dr. Henrici does not state by what means he obtains pure mercury; but it is well known to all who have much to do with this metal that it becomes very readily tarnished or dirty, and that it is troublesome to clean. Indeed from the time of Pre-vost to that of Quincke it was always an anomaly in the pheno-mena of surface-tension, that water, in which $t = 7 \cdot 3$, would not spread out into a film upon mercury, in which $t = 47$. But the fact is that in trying this important experiment no one had de-voted sufficient attention to the obtaining of chemically clean mercury until Quincke* fulfilled this necessary condition, and thus removed this anomaly from science. So also in testing the liberating action of mercury on the carbonic acid of soda-water, the results are sure to be erroneous unless special means be adopted for obtaining a pure and clean metal. I had some mercury by me that had been cleaned a few years ago; it was bright, convex, and tailless, and when last purified fragments of phosphorus would move over its surface with great freedom. Such was not the case now; the phosphorus was motionless on the surface. A portion of the metal was therefore put into a clean stoppered phial and shaken up with acid nitrate of mer-cury, and a day or two afterwards with the strongest oil of vitriol, and lastly well rinsed with clean water. The bottle was then filled up with soda-water, and the metal, as was to be expected, so far from displaying the singular activity referred to by Dr.

* Poggendorff's *Annalen*, January 1870. A translation of this paper appeared in the Philosophical Magazine for April, May, and June 1871. Quincke's method of obtaining pure mercury is given at p. 460.

Henrici, was absolutely passive, even when set in motion so as to expose fresh surfaces to the solution.

When a surface is properly cleaned by one or other of the methods pointed out, the test of the cleansing process is so delicate, that on immersing the solid in soda-water the greater or less appearance of the bubbles or their entire absence marks exactly how much, or how little, or how completely the process has been successful. For example, the middle portion of a thick platinum wire was held in the flame of a spirit-lamp until it glowed. It was then put into soda-water, when there was an abundant deposit of bubbles above and below the part that had been heated, but not a bubble on that part. A glass tube was dipped into sulphuric acid to the depth of about 2 inches; a cork was then driven into the upper end, and the tube sunk to the depth of 4 inches in the acid. It was then taken out and rinsed, the cork being removed, and so placed in soda-water to the depth of 6 inches. 4 inches of the outside and 2 inches of the inside were free from bubbles; but the remainder of the tube, both inside and out, was thickly coated with them. A glass cylinder 7 inches in height, that had been made clean by the action of strong sulphuric acid, was wiped with what would be called a clean duster to the depth of about 2 inches at the open end, and then filled up with soda-water. The part that had been wiped was accurately delineated by being covered with bubbles, while the remaining 5 inches of surface were completely free from them. In a clean glass full of soda-water the finger was introduced below the surface with friction against the side. The finger-mark became immediately apparent in consequence of the formation of bubbles upon it. It was formerly supposed that rough surfaces were particularly favourable to the liberation of gas. Such surfaces have really no action, provided they are chemically clean and not porous, as was shown in my former experiments in the case of a rat's-tail file*. But it may happen that in attempting to clean an iron or steel wire, such as a knitting-needle, in strong nitric or dilute sulphuric acid, the surface becomes graphitic; and in such a case, on placing it in soda-water, it becomes immediately covered with bubbles.

Dr. Henrici found that quartz-sand gave off numerous bubbles of gas—a clear proof that it was unclean. He also found that charcoal made red-hot and plunged into soda-water was inactive, doubtless from the absorption of gas. I obtained the same result some years ago, and also that cocoa-nut-shell charcoal and boxwood-charcoal by long keeping under water that had been boiled, were equally inactive in soda-water, since the pores were already filled, or the gas contained in them had entered into

* Phil. Mag. for August 1867. Experiments 3 and 4.

solution. But if the charcoal thus treated be taken out of the soda-water and be placed in distilled water in a flask over the flame of a spirit-lamp, the gas reassumes its elastic form, and copious torrents are poured off from it during a long time. When this is exhausted, steam takes its place; and this action may be continued for any length of time, the effect being to prevent the bumping of the vessel, and to increase the amount of vapour given off [*].

In some of Dr. Henrici's experiments it appears that while in the same gaseous solution platinum had scarcely any action, silver became coated with numerous bubbles. The reason simply was that the platinum happened to be clean and the silver unclean. So also on gently heating a solution of ammoniacal gas (*liquor ammoniæ*), no bubbles were formed either on platinum or silver. The reason was that the alkali made their surfaces clean. But if a platinum wire be drawn between the finger and the thumb previously touched with a fatty body, such as lard, the wire becomes abundantly covered with bubbles on introducing it into the solution of ammonia in a test-tube held over a spirit-lamp so as to warm it gently. The same result may be obtained with an aqueous solution of nitrous oxide under the receiver of an air-pump, a slight diminution of pressure being sufficient for the purpose.

Dr. Henrici obtained no result on placing an oil or a solid fatty body, such as stearine, on the surface of soda-water. Had he dipped a clean glass rod into oil, or rubbed it with stearine, he would have found it become abundantly covered with bubbles on immersing it in soda-water.

The fact is, that in an inquiry of this kind we cannot form clear ideas unless we distinguish and classify the bodies used according to the nature of their surfaces. And in this respect bodies may be arranged into three or four classes, in the first of which we place glass and all vitreous or vitrified surfaces, and the denser metals with a smooth non-porous surface, such as platinum, gold, silver, iron, steel, lead, tin, and mercury. To all these bodies, when chemically clean, the gaseous supersaturated solution adheres in the most perfect manner, so that there is no separation of gas at their surfaces.

In the second class we may place oils, both fixed and volatile, and fatty bodies, whether acid or neutral, various kinds of wax, resin (such as shellac and amber), camphor, phosphorus, and some other bodies, which, when chemically clean and placed in a gaseous supersaturated solution, display a very different kind of adhesion to one of its constituents as compared with the other;

* "On the Action of Solid Nuclei in liberating Vapour from Boiling Liquids," Proc. Roy. Soc. 1869, p. 240.

for while the gas adheres strongly to such bodies, the water has a much weaker adhesion. Hence, while such bodies are not wetted by the water, they are so by the gas ; and consequently in a supersaturated gaseous solution their surfaces become covered with bubbles, which increase in size, loosen their hold, and ascend, while other bubbles are formed in their place. This action goes on until the solution loses its state of supersaturation and becomes saturated only ; but in this condition the bodies in this class remained covered with the bubbles last formed upon them, and that for days together. The solution, however, can be restored to its state of supersaturation by diminishing the pressure or by increasing the temperature, in which case the liberation of bubbles from the surfaces is renewed.

It may here be remarked that bodies in class I. are said to be "dirty" or "active," or "dynamic" or "nuclear," or "uncatharized" when they are contaminated, however slightly, with any one of the bodies in class II.

The third, and by far the most numerous class, contains bodies that are distinguished by porosity—such as various kinds of wood and the charcoals made from them, and a large variety of other bodies, including some of the metals. But as the consideration of this, as well as of the fourth class, which includes soluble substances, involves a number of details, we must defer them to another occasion.

Highgate, N.
March 15, 1873.

XXXVII. *On Galvanic Induction.* By A. F. Sundell[*].

ALTHOUGH many natural philosophers have turned their attention to the phenomena of galvanic induction and endeavoured to deduce them from a common principle, yet very few experiments have been made in order to ascertain if the theories agree in all respects with experience. The object of the experiments hitherto made has been, for the most part, to find in what manner the induction depends on the form and magnitude of the circuits and the strength of the primary current. Recently Professor Edlund in Stockholm has made these phenomena the subject of a theoretical research[†], and shown that they, like all known electrical phenomena, have their origin in the luminiferous æther. He has also deduced a formula for

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[*] Communicated by the Author.
[†] "Sur la Nature de l'Electricité," *Archives des Sciences de la Bibliothèque Universelle* (Genève), 1872, Mars et Avril ; Philosophical Magazine, 1872, vol. xliv. pp. 81 and 174.

the inducing-force. It was for the purpose of verifying this formula that the experiments described in this paper were made, in the physical laboratory of the Royal Academy of Sciences, Stockholm.

1. The induction-coils were arranged in the following manner. A copper wire, 0·5 millim. thick, insulated with silk, was wound in the rectangular incision on the circumference of a circular wooden plate. Four such coils were used. Two of the plates had a diameter of 44·4 centims.; the breadth of the incision was 0·9 centim., and its depth 0·55 centim.; thus the radius of the plate, from its centre to the bottom of the incision, was 21·65 centims. The spirals of these two coils were arranged in two sets. The other two plates had a radius of 6·95 centims. to the bottom of the incision, the breadth of which was 0·6 centim., and the depth 0·3. The incisions of these two plates were wholly filled up by the wire spirals. In the centres of the plates round holes were drilled; thus the plates could be placed on a prismatic wooden bar, on one side of which a paper scale of centimetres was extended. The primary coil was fixed on the end of the bar; the secondary coil was movable along it. In every experiment the plates were placed with their plane perpendicular to the length of the bar; thus its geometrical axis passed through the centre of the coil perpendicular to the planes of its spirals.

The primary current was produced by a galvanic battery of four to six Bunsen's cells. The intensity of this current was measured by a tangent-galvanometer. The disjunctor, or the apparatus for effecting the induction, consisted of two wheels of boxwood on a common axis with a handle. The peripheries of the wheels were divided, by means of sixteen brass plates, into intervals alternately conducting and non-conducting. Two brass springs of equal length pressed on each periphery. The one pair of springs made part of the primary circuit. Thus, by turning the handle, the primary current was alternately established and destroyed; at the moment of its breaking, the other pair of springs was in contact with a brass plate of the second wheel, thus completing the secondary circuit, in which a Weber's magnetometer was inserted. But when the first pair of springs just touched a brass plate, then the second pair pressed on a non-conducting interval, and the secondary circuit was incomplete. By this arrangement we got only secondary currents of the same direction, viz. those produced by breaking the primary current.

In the experiments the handle of the disjunctor was turned once round in half a second, sixteen secondary currents being thus produced. At every complete turn a brass spring pressed

gently in an incision on the handle, whereby the experiment could be stopped after a certain number of turns. Usually five turns were made in an experiment; the amplitude of the *first* deflection that the eighty secondary currents produced in the magnetometer was taken as a relative measure of the strength of induction. The amplitude was observed with a telescope and a millimetre-scale by means of the mirror fixed to the magnetic needle. Whenever the handle was turned more or less than five times, the observed deflection was reduced to that for eighty currents. The conducting-power of the secondary circuit was measured by means of a magnet inductor. Before the commencement of an experiment the wheels of the disjunctor were always in such a position as to complete the primary circuit. The wires from the battery, the coils, and the magnetometer were twisted together two by two, or went parallel a short distance. Thus every other inducing effect, except that between the coils, was prevented.

2. We will relate the series of experiments in the order in which they were made. The following designations may be used:—i represents the intensity of the primary current; m the number of windings in the primary coil, and n that in the secondary coil; l the conducting-power of the secondary circuit, and J the first deflection of the needle of the magnetometer for eighty currents. The distance between the coils, denoted by z, is estimated from the plane of the middle winding in the one to the corresponding plane in the other coil.

Series 1. One of the large coils was taken as primary, and one of the small as secondary coil, $i = \tan 15°$, $m = 31$, $n = 51$. The current induced by the magnet inductor deflected the needle of the magnetometer eighty-seven divisions of the scale; accordingly $l = 87$.

	1.	2.	3.	4.	5.	6.
z (centimetres) . .	15	20	25	30	40	40
J (divisions of the scale)	93·4	65·2	46·0	32·8	17·8	18·0
	93·6	65·0	45·6	32·0	17·4	18·4
	93·2	. . .	. . .	32·8		
J (mean)	93·4	65·1	45·8	32·5	17·6	18·2

	7.	8.	9.	10.	11.	12.
z . .	30	25	20	15	10	1·5
J . .	35·0	48·0	67·0	93·4	127·5	176·5
	35·2	47·6	67·2	93·0	127·3	175·5
					127·5	
J (mean)	35·1	47·8	67·1	93·2	127·4	176·0

If the arithmetical mean of the deflections corresponding to

the same z is taken, we obtain the following result of this series :—

z	. .	1·5	10	15	20	25	30	40
J	. .	176·0	127·4	93·3	66·1	46·8	33·8	17·9

Series 2. Here the two greater coils were used; $i=\tan 14°$, $m=31$, $n=32$, and $l=59$.

		1.	2.	3.	4.	5.	6.	7.	8.
z	. .	100	90	80	70	60	50	40	35
J	. .	5·6	8·0	11·0	15·4	22·4	35·2	57·4	74·8
		5·6	8·2	11·2	15·2	22·6	35·4	57·6	76·0
		5·8	8·0	11·0	15·6	22·6	35·0	57·8	76·0
		5·8	8·0	11·0	15·0	22·8	. . .	. . .	75·6
J (mean)		5·7	8·1	11·1	15·3	22·6	35·2	57·6	75·6

		9.	10.	11.	12.	13.	14.	15.
z	. . .	30	25	20	20	25	30	35
J	. . .	100	134·0	185·0	183·0	134·2	101·0	74·6
		100	134·2	185·2	184·0	134·4	100·6	75·0
		100	134·0	185·2				
J (mean)	100	134·1	185·1	183·5	134·3	100·8	74·8

		16.	17.	18.	19.	20.	21.	22.
z	. . .	40	50	60	70	80	90	100
J	. . .	57·6	35·4	22·4	15·2	11·0	7·6	5·8
		57·2	34·6	22·4	15·0	10·8	7·8	5·6
J (mean)		57·4	35·0	22·4	15·1	10·9	7·7	5·7

The result is :—

z	.	20	25	30	35	40	50	60	70	80	90	100
J	.	184·3	134·2	100·4	75·2	57·5	35·1	22·5	15·2	11·0	7·9	5·7.

Series 3. Instead of the wooden bar, a cathetometer was used in this series. The secondary coil was fixed to its movable telescope-holder; the other coil was placed at the top of the vertical pillar. For the rest, the coils had the described relative position; viz. the planes of the wire spirals were parallel to each other and perpendicular to the straight line between their centres. The two smaller coils were used—$i=\tan 20°$, $m=53$, $n=46$, $l=86·1$.

		1.	2.	3.	4.	5.	6.
z	. .	10	15	20	25	30	40
J	. .	155·0	69	35·2	19·8	11·8	5·4
		154·2	69	35·0	20·0	12·0	5·6
		154·0	. .	. . .	. . .	12·0	5·6
J (mean)	154·4	69	35·1	19·9	11·9	5·5

		7.	8.	9.	10.	11.	12.
z	. .	40	30	25	20	15	10
J	. .	5·5	12·0	19·8	34·4	69·0	154·2
		5·5	11·8	20·0	34·2	68·4	154·2
						68·4	155·4
J (mean)		5·5	11·9	19·9	34·3	68·6	154·6

In the mean the deflections in this series were :—

z	. .	10	15	20	25	30	40
J	. .	154·5	68·8	34·7	19·9	11·9	5·5

3. When a galvanic current of the intensity i begins in a circuit, any one element ds of it induces in an element ds_1 of another circuit an electromotive force, for the magnitude of which Professor Edlund has deduced this expression :—

$$+ \frac{i}{r^2}(a\cos\theta + \tfrac{3}{4}kh\cos^2\theta)\cos\theta_1 ds\, ds_1, \quad . \quad . \quad (1)$$

where r signifies the distance between the elements ds and ds_1, θ the angle between the element ds and the straight line that joins the two elements, and θ_1 the angle between that line and the element ds_1; a and k are constants, and h the velocity of the æther* in the primary circuit. When the induction is to be calculated arising from the breaking of the primary current, the sign $+$ of the expression (1) must be changed into $-$. The amount of induction for any actual case is found by integrating expression (1) for the whole length of the two circuits. In special cases the result of this integration is previously known. For example, if the circuits are plane curves in such a relative position that the plane of each circuit divides the other into halves situated symmetrically to the dividing plane, the whole integral is equal to zero. If only the plane of one circuit divides the other symmetrically, but not *vice versâ*, the integral of the first term involved in the expression (1) is zero. On the other hand, the second term is of no effect if both the circuits are divided symmetrically by the same plane vertical to their own planes. As the above experiments concern this last case, we shall now examine it a little more closely. We suppose that the circuits are circles, the centres of which are on a perpendicular to their planes. This perpendicular we take for z-axis, and the plane of the primary circuit for xy-plane. Thus the origin corresponds to the centre of this circuit. Every plane through the z-axis is perpendicular to the circuits and divides them symmetrically. Therefore the integral of the second term in the expression (1)

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* According to Professor Edlund's theory of electrical phenomena, the galvanic current consists in a translative motion of the luminiferous æther in the direction of the positive current.

disappears, and the whole induction is

$$ai \iint \frac{\cos \theta \cos \theta_1}{r^2} \, ds \, ds_1. \quad . \quad . \quad . \quad (2)$$

This expression represents the induction in the *first* moment of the phenomenon. In order to obtain the electromotive force for the whole duration of the induction, the function under the signs of integration must be multiplied by a function of r, viz. br, in which b is a constant, whence results

$$abi \iint \frac{\cos \theta \cos \theta_1}{r} \, ds \, ds_1. \quad . \quad . \quad . \quad (3)$$

The axis of the y-coordinate may be taken in the negative direction through the inducing element ds; and the x-coordinate may be reckoned negative parallel to the direction of the current in ds. Integrating for ds_1, the direction along the secondary circuit is taken opposite to that of the primary current. If R is the radius of the primary circuit, R_1 that of the secondary circuit, and x, y, z are the coordinates of ds_1, we have

$$r = \sqrt{R^2 + R_1^2 + z^2 + 2Ry},$$

$$\cos \theta = -\frac{x}{r},$$

$$\cos \theta_1 ds_1 = -\frac{R}{r} \, dy,$$

and

$$abi \iint \frac{\cos \theta \cos \theta_1}{r} \, ds \, ds_1 = Rabi \iint \frac{x}{r^3} \, dy \, ds. \quad . \quad . \quad (4)$$

Observing that for two elements ds_1 of equal length and in a symmetrical position to the yz-plane the function $x \, dy$ has the same sign, and that each single element of the primary circuit has the same position relative to the secondary circuit, we can write expression (4) thus :—

$$4\pi R^2 abi \int_{-R_1}^{+R_1} \frac{x \, dy}{r^3} = 4\pi R^2 abi \int_{-R_1}^{+R_1} \frac{\sqrt{R_1^2 - y^2} \, dy}{(R^2 + R_1^2 + z^2 + 2Ry)^{\frac{3}{2}}}. \quad . \quad (5)$$

In the above experiments the windings of the coils may be regarded as circles with their centres on the z-axis and their planes parallel to the xy-plane. Expression (5) can, of course, be used for calculating the intensity of the induced electromotive force according to different values of R, R_1, and z. The spirals in a coil have not quite equal radii; nevertheless the results of the calculation will be accurate enough for comparison with the observations if a mean value for the radii be used. As

the radius of the greater coils 21·7 centims. is adopted, being the sum of the inner radius of the coil (21·65 centims.) and the thickness (0·05 centim.) of the inner set of spirals. As regards the smaller coils, to their inner radius (6·95 centims.) must be added half the depth of the incision on the peripheries (0·15 centim.); accordingly their radius is estimated at 7·1 centims. As mean values of the distance between a spiral in the one coil and one in the other coil the distances above given under z are adopted. Let the deflection produced by the induced currents in an experiment be J, we have

$$J = 4\pi c R^2 i l m n \int_{-R_1}^{+R_1} \frac{\sqrt{R_1^2 - y^2}}{(R^2 + R_1^2 + z^2 + 2Ry)^{\frac{3}{2}}} dy, \quad . \quad (6)$$

where l, m, n have the same signification as above (§ 2), and c is a constant containing a and b. We substitute a new variable,

$u = \dfrac{y}{R_1}$, and write $\qquad \dfrac{R^2 + R_1^2 + z^2}{2RR_1} = \alpha$;

thus the equation (6) changes into

$$J = \pi c \sqrt{2RR_1} \, i l m n \int_{-1}^{+1} \frac{\sqrt{1 - u^2}}{(\alpha + u)^{\frac{3}{2}}} du,$$

or

$$J = \pi c \sqrt{2RR_1} \, i l m n A, \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

if we denote by A the integral depending on α. The value of α is in all actual cases of induction greater than unity. In order to compare the different series, we have calculated the deflections J_1 corresponding to $R = R_1 = z = 1$ or $\alpha = \frac{3}{2}$, $i = 1 = \tan 45°$, $l = 1000$, and $mn = 100$. We have

$$J_1 = 100000 \pi c \sqrt{2} . A_1, \quad . \quad . \quad . \quad . \quad (8)$$

where A_1 is the value of A corresponding to $\alpha = \frac{3}{2}$. Eliminating c, we get

$$J_1 = \frac{100000}{i l m n \sqrt{RR_1}} . \frac{A_1}{A} . J. \quad . \quad . \quad . \quad (9)$$

This equation gives the value of J_1 for every observed J. The theory is confirmed if these values agree within the limits of errors of observation. Then the deflections observed with different distances between the coils may not differ in any remarkable degree from the corresponding values of J calculated by the formula

$$J = \frac{i l m n \sqrt{RR_1}}{100000} . \frac{A}{A_1} . J_1, \quad . \quad . \quad . \quad (10)$$

where the arithmetical mean of the values of J_1 in a series is t be used.

The integral A is found by developing the function in a series. As $\frac{u}{\alpha} < 1$, we have

$$\frac{1}{(\alpha+u)^{\frac{3}{2}}} = \frac{1}{\alpha^{\frac{3}{2}}}\left(1 - \frac{3}{2}\cdot\frac{u}{\alpha} + \frac{3\cdot5}{2\cdot4}\cdot\frac{u^2}{\alpha^2} - \frac{3\cdot5\cdot7}{2\cdot4\cdot6}\frac{u^3}{\alpha^3} + \ldots\right)$$

$$= \frac{1}{\alpha^{\frac{3}{2}}}\Sigma_{p=0}^{p=\infty}(-1)^p B_p\cdot\frac{u^p}{\alpha^p},$$

where

$$B_p = \frac{3\cdot5\cdot7\ldots(2p+1)}{2\cdot4\cdot6\ldots2p}. \quad \text{For } p=0, B_0 \text{ is } =1.$$

Hence

$$A = \frac{1}{\alpha^{\frac{3}{2}}}\Sigma_{p=0}^{p=\infty}(-1)^p\frac{B_p}{\alpha^p}\int_{-1}^{+1}u^p\sqrt{1-u^2}\,du. \quad . \quad (11)$$

By the theory of binomial integrals we have

$$\int u^p\sqrt{1-u^2}\,du = -\frac{u^{p-1}(1-u^2)^{\frac{3}{2}}}{p+2} + \frac{p-1}{p+2}\int u^{p-2}\sqrt{1-u^2}\,du.$$

For the limits $u=+1$ and $u=-1$ the first term in the right-hand member is zero, and

$$\int_{-1}^{+1}u^p\sqrt{1-u^2}\,du = \frac{p-1}{p+2}\int_{-1}^{+1}u^{p-2}\sqrt{1-u^2}\,du.$$

In the same manner we find

$$\int_{-1}^{+1}u^{p-2}\sqrt{1-u^2}\,du = \frac{p-3}{p}\int_{-1}^{+1}u^{p-4}\sqrt{1-u^2}\,du,$$

$$\int_{-1}^{+1}u^{p-4}\sqrt{1-u^2}\,du = \frac{p-5}{p-2}\int_{-1}^{+1}u^{p-6}\sqrt{1-u^2}\,du, \&c.$$

If p is an odd number $2q+1$, the last integral of this kind is $\int_{-1}^{+1}u\sqrt{1-u^2}\,du$; and for p even $=2q$ the last integral is $\int_{-1}^{+1}\sqrt{1-u^2}\,du$. It is easily found that the former integral is $=0$; thus all coefficients B_p with an odd index disappear. The value of the latter integral is $=\frac{\pi}{2}$. If we ascend successively to the original integral, we find for $p=2q$:—

$$\int_{-1}^{+1}u^{2q}\sqrt{1-u^2}\,du = \frac{(2q-1)(2q-3)(2q-5)\ldots3\cdot1}{(2q+2)\;2q\;(2q-2)\ldots6\cdot4}\cdot\frac{\pi}{2} = C_{2q}\cdot\frac{\pi}{2},$$

if we denote by C_{2q} the fractional coefficient, which is $=1$ for

$q = 0$. The result of the calculation is

$$A = \frac{\pi}{2\alpha^{\frac{3}{2}}} \Sigma_{q=0}^{q=\infty} B_{2q} . C_{2q} . \frac{1}{\alpha^{2q}},$$

or

$$A = \frac{\pi}{2\alpha\sqrt{\alpha}} \left[1 + \frac{1}{4} \cdot \frac{3 \cdot 5}{2 \cdot 4} \cdot \frac{1}{\alpha^2} + \frac{1 \cdot 3}{4 \cdot 6} \cdot \frac{3 \cdot 5 \cdot 7 \cdot 9}{2 \cdot 4 \cdot 6 \cdot 8} \cdot \frac{1}{\alpha^4} \right.$$
$$\left. + \frac{1 \cdot 3 \cdot 5}{4 \cdot 6 \cdot 8} \cdot \frac{3 \cdot 5 \cdot 7 \cdot 9 \cdot 11 \cdot 13}{2 \cdot 4 \cdot 6 \cdot 8 \cdot 10 \cdot 12} \cdot \frac{1}{\alpha^6} + \dots \right].$$

In calculating the values of A by this formula, the terms have been determined to four decimals. The more the value of α exceeds unity, the more rapidly does this series converge. Generally 4 or 5 terms were sufficient. Only for values of α very near unity was it necessary to calculate 6 or 7 terms besides the first. A_1 is found $= 0.7079 . \frac{\pi}{2}$; in this value are contained the first nine terms of the series. The results of the whole calculation were the following:—

Series 1. $R = 21.7$, $R_1 = 7.1$, $mn = 1581$, $l = 87$, and $i = \tan 15°$.

z.	J_1.	J.		Difference.
		Calculated.	Observed.	
1·5 . .	49·74	176·7	176·0	+0·7
10 . .	49·59	128·3	127·4	+0·9
15 . .	49·91	93·4	93·3	+0·1
20 . .	50·05	66·0	66·1	−0·1
25 . .	50·16	46·6	46·8	−0·2
30 . .	50·65	33·3	33·8	−0·5
40 . .	49·53	18·1	17·9	+0·2
Mean .	49·95			

Series 2. $R = R_1 = 21.7$, $mn = 992$, $l = 59$, and $i = \tan 14°$.

z.	J_1.	J.		Difference.
		Calculated.	Observed.	
20 . .	51·86	186·4	184·3	+2·1
25 . .	52·55	133·9	134·2	−0·3
30 . .	53·43	98·5	100·4	−1·9
35 . .	53·27	74·0	75·2	−1·2
40 . .	53·24	56·7	57·5	−0·8
50 . .	52·87	34·8	35·1	−0·3
60 . .	52·19	22·6	22·5	+0·1
70 . .	51·84	15·4	15·2	+0·2
80 . .	53·11	10·9	11·0	−0·1
90 . .	52·26	7·9	7·9	0
100 . .	50·29	5·9	5·7	+0·2
Mean .	52·45			

Series 3. $R = R_1 = 7 \cdot 1$, $mn = 2438$, $l = 86 \cdot 1$, and $i = \tan 20°$.

z.	J_1.	J.		Difference.
		Calculated.	Observed.	
10 . .	49·63	153·7	154·5	−0·8
15 . .	49·77	68·2	68·8	−0·6
20 . .	49·17	34·8	34·7	+0·1
25 . .	49·71	19·8	19·9	−0·1
30 . .	48·33	12·2	11·9	+0·3
40 . .	49·66	5·5	5·5	0

Mean . 49·38

The differences are not important; the theory is certainly confirmed in a very remarkable manner. Also the accordance of the mean value of J_1 is very satisfactory; for we have found

in the series	1.	2.	3.
J_1	49·95	52·45	49·38.

The mean value is 50·59, with the probable error ±0·636, a small quantity considering the simple means used for the experiments.

4. We will now consider the case in which the integral of the first term in expression (1) disappears. Let the induced circuit turn, about the diameter parallel to the *y*-axis, 90° from its position in the preceding experiments. Thus the two circuits come into a relative position in which no induction exists. The integral of the first term disappears, because the plane of the secondary circuit divides the primary circuit symmetrically; and as the *xz*-plane divides both the circuits symmetrically, the second term also is of no influence. But if we displace the secondary circuit in the *yz*-plane so that neither the *y*-axis nor the *z*-axis passes through its centre or intersects its circumference, the integral of the second term obtains a finite value. In order to show this, we draw through the inducing element *ds*, the coordinates of which may be *x, y*, 0, a plane (denoted by P) parallel to the *x*-axis and containing the centre of the secondary circuit. Thus the plane P divides this circuit symmetrically. We combine the element *ds* with an element ds_1 of the secondary circuit on one side of P. Let the coordinates of ds_1 be 0, η, ζ; then we find

$$\cos^2 \theta = \frac{\eta^2 x^2}{r^2 R^2},$$

and the electromotive force in the first moment

$$= + \frac{3}{4} ikh \frac{\eta^2 x^2}{r^3 R^2} \cos \theta_1 \, ds \, ds_1. \quad . \quad . \quad . \quad (12)$$

To the element 0, η, ζ corresponds another element 0, η_1, ζ_1, at the same distance from the plane of symmetry P, but on the

opposite side of it. If we combine that element with ds, we obtain the same numerical value of r and $\cos \theta_1$, but the latter quantity changes its sign. The electromotive force of this combination is

$$-\frac{3}{4}ikh\frac{\eta_1^2 x^2}{r^4 \mathrm{R}^2}\cos\theta_1\,ds\,ds_1. \quad . \quad . \quad . \quad (13)$$

By adding (12) and (13) we obtain

$$+\frac{3}{4}ikh\frac{x^2}{r^4 \mathrm{R}^2}(\eta^2-\eta_1^2)\cos\theta_1 ds\,ds_1. \quad . \quad (14)$$

If we multiply by br and effect the integration along one of the halves into which the secondary circuit is divided by the plane P, we obtain the inducing force of the element ds. By a second integration along the primary circuit, the whole amount of induction will be known. As none of the finite factors in expression (14) changes sign within the limits of integration, the integral has, mathematically speaking, a finite value. However, it is possible that the effect of the induction may be too feeble to be shown by the experiment, because the coefficient of the second term is very small in comparison with that of the first term[*]. Moreover an attempt to measure this effect directly by placing the coils so as to annul as much as possible the influence of the first term did not afford a decisive result, though a little modification of the experiment was more successful. It is evident that $\cos \theta$ and the first term in expression (1) change sign when the primary current is reversed; on the contrary, the sign of the second term, which contains $\cos \theta$ in the second degree, is independent of the direction of the current in the element ds. Accordingly, if the induction produced, for example, by breaking the primary circuit is observed for the opposite directions of the inducing current, the coils being in such a position that only the second term has an influence, the deflections of the magnetometer must be equal and towards the same side from the position of equilibrium. A perfectly accurate adjustment of the coils is not possible; therefore even the first term must be taken into account, as also other causes of magnetic deflection. Let a denote the deflection corresponding to the first term, b that for the second term, a_1 and b_1 the same for the induction between the primary coil and the coil of the multiplier and its wires, a_2 and b_2 the induction from the wires of the battery. The primary coil and the wires of the battery were not infinitely distant from the magnetometer; thus a direct influence on the magnetic needle was possible. Let c denote this influence of the coil, and c_1 that of the battery wires. The wires

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[*] Edlund, "Sur la Nature de l'Électricité."

from the disjunctor to the commutator and the coils were twisted together; and therefore their influence may be regarded as inconsiderable. The tangent-galvanometer was only occasionally inserted in the primary circuit to obtain an approximate value of the intensity of the current; in the induction-experiments it was taken out of the circuit. The whole deflection is $a + a_1 + a_2 + b + b_1 + b_2 + c + c_1$ for the one direction of the primary current, and $-a - a_1 + a_2 + b + b_1 + b_2 - c + c_1$ for the opposite direction. By addition we obtain $2(a_2 + b_2 + b + b_1 + c_1)$. The quantity of $a_2 + b_2 + c_1$ was measured by excluding the inducing coil from the primary circuit. In relation to the diminished resistance the intensity of the current was very great ($= \tan 52°$); yet the deflection (for 128 secondary currents) did not reach half a division of the scale. When the inducing coil was made part of the circuit, the strength of the current was only $= \tan 28°$; and for so feeble a current the sum $a_2 + b_2 + c_1$ may be regarded as $= 0$. In the following experiments the coils had this position : the windings of the primary coil were parallel to the xy-plane; and the middle spiral of the secondary coil was in the yz-plane, in such a position that it was not intersected by any one of the axes. For observing the effect of the second term, this relative position of the coils is the most advantageous, as the elements of the integral in expression (14) are all of the same sign. The distance of the centre of the secondary coil from the xz-plane, was a little greater than its diameter. The coils were of the described construction; their inner radius was 16 centims., the thickness of the wire 1 millim., and the number of spirals forty-five. The conducting-power of the secondary circuit was $= 119$. The deflections (for 128 currents) to rising numbers of the scale are preceded by the sign $+$, and those to the opposite side by $-$. Before every new experiment the inducing current was reversed.

Experiment 1.	2.	3.	4.
Deflections .	$+13\cdot2$	$-12\cdot0$	$+12\cdot4$	$-12\cdot6$
	$+11\cdot2$	$-12\cdot2$	$+12\cdot2$	$-13\cdot0$
	$+12\cdot8$	$-12\cdot6$	$+12\cdot2$	$-11\cdot4$
	$+12\cdot2$			
Means .	$+12\cdot4$	$-12\cdot3$	$+12\cdot3$	$-12\cdot3$

Experiment 5.	6.	7.	8.
Deflections .	$+12\cdot4$	$-12\cdot8$	$+12\cdot4$	$-12\cdot4$
	$+12\cdot0$	$-13\cdot4$	$+12\cdot0$	$-12\cdot2$
	$+12\cdot2$	$-12\cdot6$	$+12\cdot0$	$-11\cdot6$
Means .	$+12\cdot2$	$-12\cdot9$	$+12\cdot1$	$-12\cdot1$

The mean values of these deflections are $+12\cdot25$ and $-12\cdot40$.

As in comparison with the great distance between the inducing coil and the magnetometer, b_1 can be neglected, for b we may assume $2b = -12\cdot40 + 12\cdot25 = -0\cdot15$, or $b = -0\cdot08$, a quantity certainly within the limits of errors of observation. Thus these experiments confirm that the second term in the formula of induction is very small compared with the first term.

5. When the æther in a circuit is put in motion, its repulsive force on the surrounding æther-molecules is changed; and these are moved to a new position of equilibrium. Also the æther in a circuit placed in the neighbourhood of the former is momentarily moved; that is to say, a secondary current is excited in it. When the inducing current ceases, the surrounding æther and that in the secondary circuit return to the original position of equilibrium; and thus a secondary current of opposite direction is produced. It is known that secondary currents of the same intensity as those which are excited by establishing or destroying the inducing current, the circuits being in any one relative position, are also produced when the circuits are brought into this position from another (in which no induction is performed by completing or breaking the primary circuit), or *vice versâ*, from the former into the latter position. The new theory explains this in a very simple manner. Suppose the inducing current is established; then the æther in the secondary circuit is displaced from its original position of equilibrium. We can restore the original equilibrium in two ways. The cause of perturbation is removed either by breaking the inducing current or by changing, in a suitable manner, the relative position of the circuits, for example, by making the distance between them very great. Further, by bringing the circuits from this latter position into the former, the æther in the secondary circuit is disturbed in the same manner as by establishing the inducing current in the original position. The velocity of the relative motion has no influence on the total intensity of the induced currents.

In connexion with the experiments related, the following were made in order to ascertain the agreement in results of the two methods of induction. The coils last used were placed horizontally the one upon the other, so that their centres were on the same vertical line. In this position the primary circuit was (once) completed or broken, and the intensity of the excited secondary currents was noted. Then, the primary circuit being closed, the secondary coil was vertically raised to a position in which no inducing effect was observed on establishing or destroying the inducing current. The last experiment consisted in depressing the secondary coil to its original position; the intensity of the secondary currents produced by change of relative

position was also observed. The conducting-power of the secondary circuit was = 119, and the intensity of the inducing current = tan 28°. The following deflections were observed. The signs denote, as above, the side of the scale towards which the magnetic needle was deflected.

The primary circuit broken.	The secondary coil raised.
−36·0	−36·0
35·8	36·8
36·0	36·4
36·2	36·8
Mean −36·0	Mean −36·5
The circuit closed.	The coil lowered.
+36·2	+36·0
36·4	36·2
36·4	35·4
Mean +36·0	Mean +35·9

Thus, according to the theory, the intensity of the secondary current is the same for all four ways of effecting the induction.

XXXVIII. *The Vibrations which Heated Metals undergo when in contact with cold Material, treated mathematically.* **By** A. S. Davis, *M.A.*[*]

UNDER certain conditions a heated piece of metal, when laid upon a cold block of metal or some other suitable material, will vibrate by rocking rapidly about upon its points of support; and the vibrations will continue as long as the heated metal continues much hotter than the cold block upon which it rests.

One of the conditions essential for the production of these vibrations is that the heated metal must be so shaped and so placed upon the cold block that it would, if cold, rock rapidly to and fro for a short time upon being slightly tilted and left to itself. This condition may be attained by shaping the piece of metal with two parallel ridges near together, on which it may rest upon the cold block, whilst it finds a third point of support on the table upon which the block is placed. The heated piece of metal may consist of brass, copper, iron, or generally of any of those metals which are good conductors of heat. The best materials known for the cold block are lead and rock-salt.

Sir John Leslie first suggested that the cause of these vibrations is to be found in the expansion of the cold block by the heat which flows into it from the hot metal at the points of con-

tact. Faraday *, Seebeck †, and Tyndall ‡ have adopted this explanation ; and they have shown that most of the facts which they and others have ascertained respecting these vibrations are easily explained upon this view of their cause, supposing only that the expansion is sufficiently great to produce any sensible effect.

Professor James Forbes §, on the other hand, after an extensive series of experiments, was led to reject Sir John Leslie's explanation, one of his principal reasons for doing so being the impossibility, as it appeared to him, that the expansion occasioned by so slow a process as the conduction of heat could produce any sensible mechanical effect. He says, " Even at first sight it does appear very difficult to conceive how, when the vibrations are increased to 500 or more in a second, a process depending upon so slow an operation as the conduction of heat should cause the metal to expand and contract successively by a finite quantity. The effect has every appearance of being one of active and almost instantaneous repulsion, and bears no resemblance whatever to the slow mechanical elevation of the surface by the process of expansion." After remarking that such inferences are often erroneous, he enters into a closer consideration of the phenomenon, but finds no reason for altering the opinion expressed in the passage just quoted.

It thus appears to be an interesting problem to determine by calculation the amount of expansion produced in the block in any given time, and to find whether it is sufficiently great to cause the vibrations in question. This is what I have attempted to do in the present paper ; and the conclusion at which I have arrived confirms the truth of Sir J. Leslie's explanation.

I. To find the expansion produced in any given time.

It will be convenient to consider in the first place the following problem :—A large heated piece of metal is laid upon a large cold piece of the same kind of metal, the surfaces in contact being horizontal planes. To find the height through which the surface of the cold metal has been raised in any given short time.

The time being short, the depth to which the heat will have penetrated will be small ; and the pieces of metal may therefore be considered infinite in extent. We have, then, for determining the temperature v at any depth x after the time t, the following equation,

$$v = v_0 - \frac{2V}{\sqrt{\pi}} \int_0^{\frac{x}{2\sqrt{kt}}} \epsilon^{-z^2} dz, \quad \dots \quad (1)$$

* Proc. of Roy. Inst. vol. ii. p. 119.　　　† Pogg. *Ann.* vol. li.
‡ Proc. of Roy. Inst. 1854.　See also ' Heat as a mode of Motion,'
p. 127.
§ Phil. Mag. vol. iv.

where V is half the difference between the initial temperatures of the two pieces of metal, v_0 is their arithmetic mean, k is the conductivity of the metal in terms of the thermal capacity of the unit of bulk. (See Thomson and Tait, 'Natural Philosophy,' vol. i. p. 717.)

If O Y be taken in the surface of the metal and O X vertically downwards, and if we take O A to represent V, and N P to represent $v - (v_0 - V)$ (*i. e.* the excess of the temperature of the metal at the depth O N $(= x)$ at the time t over the initial temperature of the metal), the curve A P Q will be very near the axis of X at all

depths greater than $4\sqrt{kt}$, because the value of the integral $\frac{1}{\sqrt{\pi}}\int_0^w \epsilon^{-z^2}dz$ is very nearly equal to $\frac{1}{2}$ for all values of $w > 2$.

Take O B $= 2\sqrt{kt}$, and complete the rectangle O D.

The expansion which the metal has undergone will be proportional to the area O A Q; and this expansion bears the same proportion to the expansion which the metal would undergo if heated uniformly to a temperature v_0 down to a depth O B as the area O A Q bears to the area of the rectangle O D. The ratio which the area O A Q bears to O D is constant, and by careful measurement is found to be nearly as 6 : 10. (This curve is carefully drawn in Thomson and Tait's 'Natural Philosophy,' vol. i. p. 719.)

Let d be the increase of unit length of the metal for an increase of one degree in the temperature. Then the height through which the surface has risen in time t is

$$\cdot6 \times V \times 2\sqrt{kt} \times d. \quad . \quad . \quad . \quad . \quad (2)$$

When the hot and cold metals are of the same kind, the contraction of the hot metal will be equal to the expansion of the cold metal (supposing the dilatability not to vary with the temperature). In this case, then, the mass of hot metal will not be raised by the flow of heat.

Let us next consider the case in which the hot and cold metals are of different kinds. The common temperature of the surfaces in contact will not be, as in the former case, a mean between the initial temperatures of the two metals.
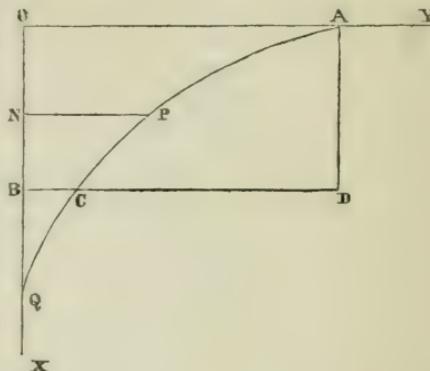
To find what this temperature will be, let T be its excess over the initial temperature of the cold metal, and let $T_{,}$ be the excess of the initial temperature of the hot metal over the same temperature. Then if $d, d', \rho, \rho', \sigma, \sigma'$ be respectively the dilatabilities, specific gravities, and specific heats of the cold and hot metals, it will be seen from what has been already proved that the expansion of the cold metal is to the contraction of the hot metal as

$$T d \sqrt{kt} : (T_{,} - T) d' \sqrt{k't}.$$

But if h be the heat which has flowed out of the hot metal into the cold metal, the expansion of the cold metal is $\dfrac{hd}{\rho\sigma}$; for the total expansion would be the same however the heat were distributed; and it is clear that $\dfrac{hd}{\rho\sigma}$ would be the expansion if the heat h were uniformly distributed over a unit cube of the metal. In the same manner the contraction of the hot metal will be $\dfrac{hd'}{\rho'\sigma'}$. Thus we have

$$T d \sqrt{kt} : (T_{,} - T) d' \sqrt{k't} : : \frac{hd}{\rho\sigma} : \frac{hd'}{\rho'\sigma'},$$

whence

$$T = T_{,} \times \frac{\rho'\sigma'\sqrt{k'}}{\rho\sigma\sqrt{k} + \rho'\sigma'\sqrt{k'}}. \quad . \quad . \quad . \quad . \quad (3)$$

When the two metals are of different kinds, the expansion of the one will not in general be equal to the contraction of the other, and consequently the mass of hot metal will be either raised or lowered.

Let us now consider the case in which the hot metal is copper and the cold metal lead. In this case

$$\rho = 11 \cdot 35 \qquad\qquad \rho' = 8 \cdot 88$$
$$\sigma = \quad \cdot 0314 \qquad\qquad \sigma' = \quad \cdot 095$$
$$d = \quad 0 \cdot 000028 \qquad\qquad d' = 0 \cdot 000017.$$

Taking a decimetre as unit of length, and a second as unit of time, and 1° C. as unit of temperature,

$$\sqrt{k} = \cdot 0327, \quad \sqrt{k'} = \cdot 0475.$$

Substituting, we obtain

$$T = \cdot 77 \times T_{,}.$$

Hence the expansion of the lead is

$$1 \cdot 2 \times 0 \cdot 000028 \times \cdot 0327 \times \cdot 77 \times T_{,} \times \sqrt{t}$$
$$= \cdot 000000846 \times T \times \sqrt{t};$$

and the contraction of the copper is

$$1\cdot2 \times 0\cdot000017 \times \cdot0475 \times \cdot23 \times T_{I} \times \sqrt{t}$$
$$= \cdot000000223 \times T_{I} \times \sqrt{t}.$$

Therefore the copper is raised in time t through a space

$$\cdot000000623\,T_{I}\sqrt{t}\ \text{decimetre},$$

or

$$\cdot000062\,T_{I}\sqrt{t}\ \text{millimetre}.$$

Let us now consider the case in which the area of the surfaces in contact is very small, which will be the case applicable to the rocker and block.

If the heat which flows out of the hot metal and into the cold metal were wholly confined to those parts which are vertically above and below the surfaces in contact, then the height to which the surfaces would rise would be given by the expression already found. But in general a certain portion of the heat will flow laterally; and consequently the height to which the surfaces rise will in general be less than the expression already found. The proportion of heat which escapes laterally will be greater the greater the depth to which the heat has flowed.

Thus let $a\,a$ be the part of the surfaces in contact. When the heat has flowed to the depth $q'\,q'$, the whole amount of heat which has escaped laterally bears a larger proportion to the heat which has flowed directly downwards than it does when the heat has only flowed to the depth $q\,q$. It will be seen also that the pro-

portion of heat which escapes laterally depends upon the ratio of the diameter of the surface in contact to the depth to which the heat has flowed.
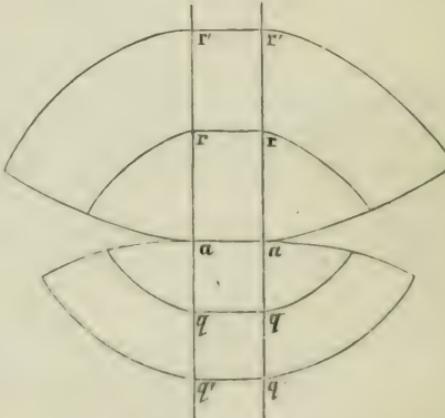
Assuming, as a probable value, $\cdot2$ millim. for the diameter of the surface in contact, and supposing that the vibrations are taking place at the rate of 225 per second, let us inquire what, under these circumstances, will be the proportion of heat which flows laterally. It will be seen from the figure that only a small part of the heat which has flowed into the lead has penetrated to

a depth greater than $2\sqrt{kt}$, and that two thirds of the whole amount is confined to a depth less than $\sqrt{kt}$. Hence, in the case under consideration, only a small part of the heat has penetrated ·44 millim. into the lead, and two thirds of the whole amount has not penetrated ·22 millim. In the same way it will be found that but little heat has flowed out of the copper from a depth greater than ·63 millim., and that two thirds of the whole amount has come from a depth less than ·32 millim. If, then, we draw the figure so that *a a, a q, a q', a r, a r'* are proportional respectively to the numbers 20, 22, 44, 32, 63, we may, by considering the proportions which the masses of heated and cooled metal vertically above and below the surfaces in contact bear to the whole masses of heated and cooled metals respectively, form a rough estimate of the proportion of heat which is available in raising the rocker.

Taking one fifth of the heat which flows into the lead as available in raising the rocker, and one tenth of that which flows out of the copper as available in diminishing the height to which the rocker is raised, the height through which the rocker is raised will be

$$\cdot0000147 \times \mathrm{T}_{,} \times \sqrt{t} \text{ millim.*,} \quad \cdot \quad \cdot \quad \cdot \quad (4)$$

when t has a value about $\frac{1}{225}$.

II. Taking this as the correct expression for the height through which the rocker is raised, I now proceed to determine the difference of temperature between the copper and lead necessary to produce continuous vibrations at a given rate in a rocker of given shape.

Let the rocker be a rectangular parallelopiped with two parallel ridges on its underside. Let the point of support which rests upon the table be so far removed from the body of the

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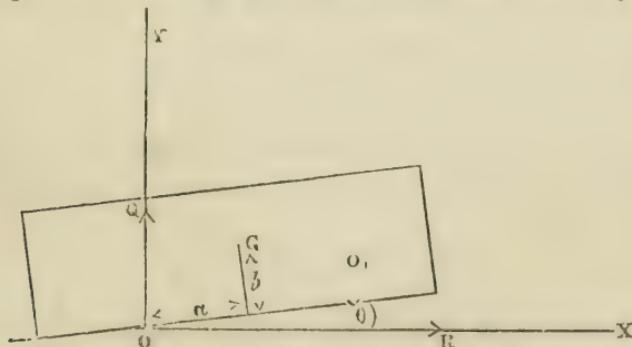
* I here assume that the total flow of heat into the lead is the same as if there were no lateral flow of heat. The fact that a lateral flow of heat occurs will probably increase the total flow, and will also probably have the effect of diminishing the depth to which the heat flows in a given time. For each of these reasons the value found above would be too small.

rocker by means of a long thin rod, and so arranged, that every vertical slice of the body of the rocker has as nearly as possible the same motion. Let M be the mass of the rocker, Mk^2 the moment of inertia about an axis through the centre of gravity parallel to the length of the rocker, $2a$ the distance between the ridges, $2b$ the depth, and $2c$ the breadth of the rocker. Let ω, ω' be the angular velocities just before and just after the impact of the rocker upon the point O. Let R, Q be the horizontal and vertical components of the impulse, u, u' the horizontal, v, v' the vertical component velocities of the centre G, ω, ω' the angular velocities of the rocker before and after impact, θ the inclination of the rocker. The equations of impulsive motion are

$$M(u'-u) = R,$$
$$M(v'-v) = Q,$$
$$Mk^2(\omega'-\omega) = R \cdot y - Q \cdot x.$$

Eliminating R and Q, we have

$$k^2(\omega'-\omega) = y(u'-u) - x(v'-v). \quad . \quad . \quad . \quad (5)$$

If x, y be the coordinates of G, and H the height of O' above O X, the geometrical relations are, before impact,

$$2a - x = a\cos\theta + b\sin\theta,$$
$$y - H = b\cos\theta - a\sin\theta;$$

whence, by differentiation,

$$-u = (-a\sin\theta + b\cos\theta)\omega,$$
$$v - \frac{dH}{dt} = (-b\sin\theta - a\cos\theta)\omega.$$

After impact the geometrical relations are

$$x = a\cos\theta - b\sin\theta,$$
$$y = a\sin\theta + b\cos\theta;$$

whence

$$u' = (-a\sin\theta - b\cos\theta)\omega',$$
$$v' = (\ \ a\cos\theta - b\sin\theta)\omega'.$$

Substituting these values in (5) and neglecting very small quantities, we obtain

$$\omega = \frac{k^2 + b^2 + a^2}{k^2 + b^2 - a^2} \times \omega'. \quad . \quad . \quad . \quad . \quad (6)$$

We have now to determine the motion between two impacts. If the vibrations are continuous, the angular velocity acquired just before the next impact upon the other point of support O' must be equal to $-\omega$. Immediately after the impact upon O, the point O begins to rise. Let h be the height to which it has

risen in the time t. Let R′, Q′ be the horizontal and vertical components of the pressure upon the rocker at the point O at the time t. Then the equations of motion are

$$M \frac{d^2x}{dt^2} = R',$$

$$M \frac{d^2y}{dt^2} = Q' - Mg,$$

$$Mk^2 \frac{d^2\theta}{dt^2} = R'y - Q'x.$$

The geometrical relations are

$$x = a \cos \theta - b \sin \theta,$$

$$y = b \cos \theta + a \sin \theta + h.$$

From these equations we obtain

$$k^2 \frac{d^2\theta}{dt^2} = -x \left(g + \frac{d^2h}{dt^2} \right)$$

$$= -a \left(g + \frac{d^2h}{dt^2} \right) \text{ very nearly.}$$

Integrating and noticing that when $t = 0$, $\frac{d\theta}{dt} = \omega'$, we have

$$k^2 \frac{d\theta}{dt} = -agt - a \cdot \frac{dh}{dt} + k^2\omega'. \quad . \quad . \quad . \quad . \quad (7)$$

Integrating again and noticing that when $t = 0$, $h = 0$, and $\theta = \frac{H}{2a}$, we have

$$k^2 \cdot \theta = -\frac{agt^2}{2} - ah + k^2\omega't + k^2 \cdot \frac{H}{2a}.$$

Now, just before the next impact O will have risen to a height H, and O′ will have sunk down to its original position. Therefore the values of θ and $\frac{d\theta}{dt}$ will be respectively $-\frac{H}{2a}$ and $-\omega$. Hence, if π be the period of the vibration, we have

$$\frac{k^2 \cdot H}{a} = \frac{ag\pi^2}{2} + a \cdot H - k^2\omega'\pi;$$

$$\therefore \omega' = \frac{a^2g \cdot \pi^2 + 2H(a^2 - k^2)}{2ak^2\pi}. \quad . \quad . \quad (8)$$

From (7) we have

$$k^2(\omega + \omega') = ag\pi + a \frac{dh}{dt}. \quad . \quad . \quad . \quad . \quad (9)$$

From (8) and (9) we obtain

$$\frac{\omega - \omega'}{\omega + \omega'} = \frac{a^2\pi \dfrac{dh}{dt} + 2\,\mathrm{H}\,(k^2 - a^2)}{a^2 g \pi^2 + a^2 \dfrac{dh}{dt}\,\pi}\,.$$

But from (6),

$$\frac{\omega - \omega'}{\omega + \omega'} = \frac{a^2}{k^2 + b^2}\cdot$$

Also

$$\mathrm{H} = \cdot 0000147 \times \mathrm{T}_{\prime}\sqrt{\vec{\pi}},$$

and

$$\frac{dh}{dt} = \frac{\cdot 0000074 \times \mathrm{T}_{\prime}}{\sqrt{\pi}}\,;$$

$$\therefore \frac{a^2}{k^2 + b^2} = \frac{\cdot 0000147 \times \mathrm{T}_{\prime}\left(\dfrac{k^2}{a^2} - 3\right)}{2 g \pi^{\frac{3}{2}} + \cdot 0000147 \times \mathrm{T}_{\prime}}\,.$$

In the rocker I have employed, $a = 2\cdot 5$, $b = 22$, $c = 10$ millims.

Putting also $g = 9800$ and $\pi = \frac{1}{2\,2\,5}$ and reducing, we obtain

$$\mathrm{T}_{\prime} = 10^{\circ}\ \mathrm{C.\ very\ nearly.}$$

Thus we conclude that if, after each impact, the heated lead and the cooled copper in the neighbourhood of the point of contact were to regain their initial temperatures before the next impact upon the same point took place, then a difference of only 10° C. between the two metals would be sufficient to keep up a vibration of the rocker at the rate of 225 impacts per second. This, of course, will not be the case; but it is not difficult to see that, owing to the fact that the little masses of heated lead and cooled copper are each half surrounded by metal, the reduction of the temperature of the metals to the temperature of the surrounding metal by the process of conduction will take place very rapidly. Thus if, just after the contact is broken, the heat has spread through a portion of the lead (approximately hemispherical) to a depth d, say, then before the contact is again made the heat derived from the previous contact will have spread to a depth $\sqrt{2}\,.\,d$, and will occupy a space approximately $2\sqrt{2}$ times that which it occupied when the last contact was broken. Consequently the increment of temperature produced by the previous contact will be diminished to $\dfrac{1}{2\sqrt{2}}$ of its former amount.

In the same manner the increment of temperature gained by the last contact but one will be diminished to about one eighth of its former amount. Only a very few of the previous contacts will therefore have any appreciable effect in causing the temperatures of the two metals at their points of contact to differ by a

smaller amount than the temperatures of the general masses of the two metals. A difference then of 50° or 100° in the temperatures of the two metals will probably be quite sufficient to produce the vibrations in question.

It has already been seen that the larger the areas in contact, the greater the proportion of heat available for raising the block. For this reason large areas in contact are advantageous for the production of vibrations. But, on the other hand, the larger the areas in contact, the more slowly will the two metals tend to regain their original difference of temperature during the intervals when they are not in contact ; and for this reason large areas in contact are disadvantageous for the production of vibrations. There will therefore be a certain magnitude for the area in contact which will give stronger vibrations than either larger or smaller areas. This conclusion is borne out by experiment.

I have pointed out that when the hot and cold metals are of the same material there will in general be no vibration, because the contraction of the hot metal will exactly compensate for expansion of the cold metal. This, as a result of his experiments, Forbes has stated to be a general law. There is, however, an exception to this law, first noticed and explained by Professor Tyndall, to which I should refer. He found that when the hot rocker rests upon the edges of thin sheets or upon points of the same metal as itself, vibrations will often occur. The explanation is to be found in the fact that, while a lateral flow of heat out of the hot metal takes place, the lateral flow of heat into the cold metal is partially prevented.

Another general result which Forbes deduced from his experiments is, that " the vibrations take place with an intensity proportional (within certain limits) to the difference of the conducting-powers of the metals for heat, the metal having the least conducting-power being necessarily the coldest." Now, if the various metals differed from one another only as regards their conductivities this law would be strictly true ; for in this case the possibility of vibration could only arise from the lateral flow of heat out of the hot metal being greater than the lateral flow into the cold metal, and this would only be the case when the hot metal was the better conductor of heat. But in any case the lateral flow of heat has an important influence upon the vibrations ; and consequently the above law will be approximately true.

If, however, the lateral flow of heat into the cold metal be prevented by making the rocker rest upon thin sheets or points of cold metal, this law will altogether fail. The experiments of Tyndall prove that this is the case.

Leeds Grammar School,
    March 6, 1873.

written at the suggestion of a friend during a Committee held in the Royal Society's Apartments, and, as the concluding words show, was intended to be followed by a more detailed account of the method of observation. The point in question relates to the position of a second slit which was used to screen the eye from every part of the spectrum except that under observation. The words in my book written at the time are, " narrow slit found to be best at focus of little telescope with positive eyepiece." In the note the second slit was stated to have been placed before the object-glass of the little telescope. Such an arrangement was tried in connexion with some other experiments in progress at the time. The plan of limiting the field of view to the part of the spectrum corresponding to the refrangibility of the light of the prominence, as well as the employment of a ruby glass, is of value when the air is not favourable, or when a spectroscope of small dispersive power is used.

Jan. 30.—George Busk, Esq., Vice-President, in the Chair.

The following communication was read :—

"On Just Intonation in Music ; with a description of a new Instrument for the easy control of all Systems of Tuning other than the ordinary equal Temperament of twelve divisions in the Octave." By R. H. M. Bosanquet, Fellow of St. John's College, Oxford.

The object of this communication is to place the improved systems of tuning within the reach of ordinary musicians ; for this purpose the theory and practice are reduced to their simplest forms.

A notation is described, adapted to use with ordinary written music, by which the notes to be performed are clearly distinguished.

The design of a key-board is described, by which any system of tuning, except the ordinary equal temperament, can be controlled, if only the fifths of the system be all equal. The design is on a symmetrical principle ; so that all passages and combinations of notes are performed with the same handling, in whatever key they occur.

The theory of the construction of scales is then developed ; and a diagram is given, from which the characteristics of any required system can be ascertained by inspection.

An account is then given of the application of such systems to the new key-board, and particularly of an harmonium which has been constructed and contains at present the division of the octave into fifty-three equal intervals in a complete form. Rules for tuning are given.

Finally, the application of the system of fifty-three to the violin is discussed.

Throughout, the work of former labourers in the same field is reviewed : the obligations of the writer are due to Helmholtz, the late General T. Perronet Thompson, F.R.S., and others.

Feb. 13.—Rear-Admiral Richards, C.B., Vice-President, in the Chair.

The following communication was read :—

"On a new Relation between Heat and Electricity." By Frederick Guthrie.

It is found that the reaction between an electrified body and a neighbouring neutral one, whereby the electricity in the neutral body is inductively decomposed and attraction produced, undergoes a modification when the neutral body is considerably heated.

Under many circumstances it is found that the electrified body is rapidly and completely discharged. The action of discharge is shown to depend mainly upon the following conditions :— (1) the temperature of the discharging body, and its distance from the electrified one ; (2) the nature (+ or —) of the latter's electricity.

With regard to (1), it is shown that the discharging power of a hot body diminishes as its distance increases, and increases with its temperature ; but, concerning the temperature, it is proved that the discharging power of a hot body does not depend upon the quantity of heat radiated from it to the electrified body, but chiefly upon its quality. Thus a white-hot platinum wire connected with the earth may exercise an indefinitely greater discharging power, at the same distance, than a large mass of iron at $100°$ C., though the latter may impart more heat to the electrified body.

Neither the mere reception of heat, however intense, by the electrified body, unless the latter have such small capacity as to be itself intensely heated, discharges the electricity if the source of heat be distant ; nor is discharge effected when the electrified body and a neighbouring cold one are surrounded by air through which intense heat is passing. But, for the discharge, it is necessary that heat of intensity pass to the electrified body from a neutral body, within inductive range.

White- and red-hot metallic neutral bodies exercise this discharging power even when isolated from the earth, but always with less facility than when earth-connected.

The hotter the discharging body, whether isolated or earth-connected, the more nearly alike do + or — electricities behave in being discharged ; but at certain temperatures distinct differences are noticed. The — electricity, in all cases of difference, is discharged with greater facility than the +.

Attempts are made to measure the critical temperatures at which earth-connected hot iron (1) discharges + and — electricity with nearly the same facility, (2) begins, as it cools, to show a preferential power of discharging —, and (3) ceases to discharge —. The temperatures so obtained are measured by the number of heat-units, measured from $0°$ C., in 1 gram of iron of the respective temperature, represented by the value of the expression $Fe \; \Sigma_0^t u$.

It is shown that various flames, both earth-connected and

isolated, have an exceedingly great power of discharging both kinds of electricity.

The effects in regard to discharge are shown to be similar when platinum wire, rendered hot by a galvanic current, is used, and also when the condensed electricity of a Leyden jar is experimented on.

As hot iron shows a preferential power of discharging — over + electricity, so it is found that white-hot but isolated iron refuses to be charged either with + or — electricity. As the iron cools, it acquires first the power of receiving —, and afterwards of receiving +. Further, while white-hot iron in contact with an electrified body prevents that body from retaining a charge of either kind of electricity, as it cools it permits a + charge to be received, and subsequently a — one.

A suggestion is made as to the existence of an electrical coercitive force, the presence of which together with its diminution by heat would explain much of what has been described.

Feb. 20.—Rear-Admiral Richards, C.B., Vice-President, in the Chair.

The following communication was read :—

"On a new Locality of Amblygonite, and on Montebrasite, a new Hydrated Aluminium and Lithium Phosphate." By A. O. Des Cloizeaux.

A mineral found in 1862 at Hebron, Maine, U. S. A., after a mere tentative examination by Professor Brush, who announced the presence in it of lithia in considerable quantity, resembled the amblygonite of Penig so closely as to lead to its being looked on as amblygonite. The crystalline system and birefringent optical characters of this mineral were determined by the author in 1863. In 1870 a mineral found in the tin vein of Montebras (Creuse), though resembling the amblygonite of Hebron, appeared to the author to differ from it so far as to justify his designation of it under the name of Montebrasite. Towards the close of 1871 he received another specimen from Montebras, which presented all the characters of the American amblygonite, and which consequently was easily distinguished from the montebrasite. Subsequently, analyses by Pisani, v. Kobell, and Rammelsberg, and optical observations by the author, proved the identity of the montebrasite of Montebras with the amblygonite from Penig. But this is not the case with the amblygonite from Hebron, nor with that from Montebras, which had been analyzed by Pisani. These differ from the amblygonites of Saxony and Montebras (which last he had previously named montebrasite) by the absence of soda, by the preponderance of lithia, and the presence of a notable amount of water, while at the same time they contain almost equal proportions of phosphoric acid and alumina.

The differences which these two minerals present in their physical and chemical characters are sufficiently decided to compel our treating them as distinct species. The name amblygonite

should be retained for the sodolithic species first discovered at Penig by Breithaupt; and the white or violet-tinted lamellar masses abundant at Montebras will be included under it; the hydrated and entirely lithic species, comprising the laminar specimens and the crystals from Maine, as well as some greenish masses from Montebras, should be embraced under the name montebrasite.

The amblygonite of Montebras has only been met with in laminar masses with a faint tinge of violet. These masses exhibit two cleavages presenting nearly the same degree of facility, making with one another an angle of 105° 44'. Close observation shows that the sharpness of the reflected images is generally a little greater on one of the cleavages than on the other; and this induces one to suppose that they do not both belong to equivalent crystallographic planes. The study of some of their optical properties, though presenting certain special difficulties, arising from the small extent of the transparent portions and the presence of numerous twin plates, even in the specimens that to all appearance are the most homogeneous, has proved that the laminar masses of montebrasite must be referred to the triclinic system. The optic axes are situated in a plane which divides into two very unequal parts the acute angle of 74° 16' of the two cleavages. This direction is entirely different from that found for montebrasite of Hebron and of Montebras, in which the plane of the axes lies in the obtuse angle of 105° formed by the two principal cleavages.

The appearance of the bars traversing the central ring of each system indicates very distinctly a twisted dispersion, as well as a small amount of inclined dispersion, which is characteristic of a crystal belonging to the triclinic system.

In November 1871 the author received a specimen from the middle of a mass of amblygonite from Montebras resembling the mineral from Hebron. It has three principal cleavages, p, m, t, which the author recognized in the mass from Hebron, the angles between which are $p\ m = 105°$, $m\ t = 135°$ to $136°$, $p\ t = 89°$ to $89°\ 15'$.

By means of artificial twins formed of two plates, each of which had been worked perpendicular to the two cleavages p and m, and which were united by their faces p, it appeared that the plane of the optic axes is situated in the obtuse angle $p\ m$, and traverses the edge $\dfrac{P}{m}$, but that it is not quite normal to m, since it gives angles of about 82° with m and 23° with p. The character of the coloured rings shows that in montebrasite of Montebras, as in that from Hebron, there coexists with the horizontal a well-marked inclined dispersion; and these are peculiar to crystals of the triclinic system.

Analyses by M. Pisani.

	Hebron.	Montebras.
Fluorine	5·22	3·80
Phosphoric acid	46·65	47·15
Alumina	36·00	36·90
Lithia	9·75	9·84
Water	4·20	4·75
	101·82	102·44
Specific gravity	3·03, Pisani.	3·01, Pisani.
	2·99, Damour.	2·977, Damour.

Wavellite, in the form of thin coatings, forms a layer over almost all the fissures that occur in the amblygonite of Montebras. In cavities in these coatings are found long thin needles, which have enabled the author to correct the older measurements of this mineral.

M. Pisani has ascertained that the variety from Montebras yields :—

Fluorine	2·27
Phosphoric acid	34·30
Alumina	38·25
Water	26·60
	101·42
Specific gravity	2·33

GEOLOGICAL SOCIETY.

November 6, 1872.—Prof. Ramsay, F.R.S., V.P., in the Chair.

The following communications were read :—

1. " A Report by F. T. Gregory, Esq., Mining-land Commissioner in Queensland, on the recent discoveries of Tin-ore in that Colony."

According to this report, the district in Queensland in which tin-ore has been discovered is situated about the head-waters of the Severn river and its tributaries, comprising an area of about 550 square miles. The district is described as an elevated granitic tableland intersected by ranges of abrupt hills, some attaining an elevation of about 3000 feet above the sea. The richest deposits are found in the beds of the streams and in alluvial flats on their banks, the payable ground varying from a few yards to five chains in extent. The aggregate length of these alluvial bands is estimated at about 170 miles ; the average yield per linear chain of the stream-beds at about ten tons of ore (cassiterite).

Numerous small stanniferous lodes have been discovered, but only two of much importance—namely, one near Ballandean Head Station on the Severn, and another in a reef of red granite rising in the

midst of metamorphic slates and sandstones at a distance of about
six miles. The lodes run in parallel lines bearing about N. 50° E.;
and one of them can be traced for a distance of nine or ten miles. The
ore, according to Mr. Gregory and Mr. D'Oyly Aplin, is always asso-
ciated with red granite—*i. e.* "the felspar a pink or red orthoclase,
and the mica generally black; but when crystals of tin-ore are found
in situ, the mica is white." The crystals of tin-ore are generally
found in and along the margins of quartz threads or veins in bands
of loosely aggregated granitoid rock, but are sometimes imbedded
in the micaceous portions. The report concludes with some state-
ments as to the present condition and prospects of the district as
regards its population.

2. "Observations on some of the recent Tin-ore Discoveries in
New England, New South Wales." By G. H. F. Ulrich, Esq.
F.G.S.

The district referred to by the author is in the most northern part
of the colony of New South Wales, almost immediately adjoining
the tin-region of Queensland described in the preceding report. It
forms a hilly elevated plateau, having Ben Lomond for its highest
point, nearly 4000 feet above the sea-level. The predominant rocks
are granite and basalt, enclosing subordinate areas composed of
metamorphic slates and sandstones; the basalt has generally broken
through the highest crests and points of the ranges, and spread in
extensive streams over the country at the foot.

The workings of the Elsmore Company, situated on the north-
west side of the Macintyre river, about twelve miles E. of the town-
ship of Inverell, include a granite range about 250 feet in height
and nearly two miles in length. The granite of the range is micaceous,
with crystals of white orthoclase, and is traversed by quartz-veins
which contain cassiterite in fine druses, seams, and scattered crystals,
and by dykes of a softer granite, consisting chiefly of mica, and with
scarcely any quartz, in which cassiterite is distributed in crystals,
nests, and bunches, and also in irregular veins several inches
in thickness. This granite yields lumps of pure ore up to at least
50 lbs. in weight. The quartz-veins contain micaceous portions
which resemble the "Greisen" of the Saxon tin-mines. The deepest
shaft sunk in one of the quartz-veins was about 60 feet in depth.
The author noticed certain minerals found in association with the
tin-ore, and the peculiarities of the crystalline forms presented by
the latter.

The drift is very rich, and consists of a generally distributed
recent granitic detritus, from 6 in. to 2 ft. thick, and of an older
drift (probably Pliocene) capping the top of the range, and probably
dipping beneath the adjoining basalt. The washing of the granitic
detritus gives from 3 ozs. to more than 2 lbs. of ore per dish (of
about 20 lbs.). The older drift is rather poor in tin to within about
a foot of the bottom; but the bottom layer is in part very rich, some
having yielded as much as 6 lbs. of ore per dish.

The author also described the Glen Creek, about 40 miles north

of the Elsmore mine, from the surface-deposits of which tin-ore has been obtained by washing. The course of the creek is mostly through a black, hard slate destitute of fossils; but at one part, for about 10 chains, its bed consists of a fine-grained hard granite, with numerous veins of arsenical and copper pyrites, and one solid vein of tin-ore, about $\frac{3}{4}$ in. in thickness, all of which pass from the granite into the slate without any interruption or change, the passage from one rock into the other being also gradual.

The chief underlying rock of the district is a black slate; but dispersed through it are small outcrops of a rather coarse-grained micaceous granite, close to one of which several veins of solid tin-ore, from 1 to 4 inches thick, have been found traversing the slate rock. The tin-ore disseminated through the surface-deposits has been derived from these veins and from a very hard and tough greenstone (diabase), which occurs in large dykes and patches in various places, and is probably younger than the granite.

In conclusion the author referred to the probability that a deficiency of water may prove a great obstacle to the full development of the tin-mining industry in this district, but stated that "it seems not unlikely that the production of tin-ore from this part of Australia will reach, if not surpass, that of all the old tin-mining countries combined."

3. "On the included Rock-fragments of the Cambridge Upper Greensand." By W. Johnson Sollas and A. J. Jukes-Browne.

The occurrence of numerous subangular fragments in the Upper Greensand formation was so far remarkable that it had already attracted the notice of two previous observers (Mr. Bonney and Mr. Seeley), who had both briefly hinted at the agency of ice. While ignorant of the suggestions of these gentlemen, the authors of this paper had been forced to the same conclusion. A descriptive list had been prepared of the most remarkable of the included fragments. The infallible signs of the Upper Greensand origin consisted in incrustations of *Plicatula sigillum*, *Ostrea vesiculosa*, and "Coprolite," without which, it was stated, the boulders would be undistinguishable from those of the overlying drift. The following generalizations were then put forword:—

1. The stones are mostly subangular; some consist of friable sandstones and shales, which could not have borne even a brief journey over the ocean-bed.

2. Many are of large size, especially when compared with the fine silt in which they were imbedded; the stones and silt could not have been borne along by the same marine current.

3. The stones are of various lithological characters, and might be referred to granitic, schistose, volcanic and sedimentary rocks, probably of Silurian, Old Red Sandstone, and Carboniferous age.

Such strata are not found *in situ* in the neighbourhood; and the blocks must have come from Scotland or Wales. Numerous arguments were adduced in favour of their Scottish derivation.

The above considerations, that numerous rock-fragments, some of

which are very friable, have been brought from various localities and yet retain their angularity, were thought sufficient evidence for their transportation by ice. The majority showed no ice-scratches; but the small proportion of scratched stones in the moraine matter borne away on an iceberg, and the small percentage of ice-scratched boulders in many deposits of glacial drift, show that the absence of these striæ is not inconsistent with the glacial origin of the included fragments. Besides this, the stones of the Greensand consisted of rock from which ice-marks would readily have been removed by the action of water. The authors stated, however, that they had found more positive evidence in a stone which was unmistakably ice-scratched, consisting of a siliceous limestone, and preserved in the Woodwardian Museum. The fauna, so far as it proved any thing, suggested a cold climate; though abundant, the species were dwarfed, in striking contrast to those of the Greensand of Southern England and the fauna of the succeeding Chalk. The authors concluded that a tongue of land separated the Upper-Greensand sea into two basins, the northern of which received icebergs from the Scottish-Scandinavian chain; the climate of this was cold, that of the southern basin much warmer.

XL. *Intelligence and Miscellaneous Articles.*

ON THE ELECTRICAL RESISTANCE OF METALS. . BY M. BENOIST.

IT has long been known that the electrical resistance of metals increases as their temperature rises. This increase has been measured up to 100° by M. Becquerel and by Matthiessen, and to 200° in some metals by M. Lenz and, more recently, by M. Arndtsen. I proposed to myself to trace the variation beyond these limits, and to determine the increment of specific resistance at very high temperatures.

Calling x the specific resistance of a metal (that is to say, its resistance in unit length and unit section), the resistance of a wire of the same metal of length l and section s is, according to Davy's laws,

$$R = x\frac{l}{s},$$

or, substituting for s its value as a function of the volume V, weight P, and density D, of the wire,

$$R = \frac{xl^2}{V} = \frac{xDl^2}{P}.$$

If D, P, and l are known, and if R at $t°$ be determined, the value of the specific resistance at that temperature can be deduced from the last relation.

To measure R, I have chiefly employed the differential-galvanometer method of M. Becquerel. The current from two Daniell's elements was divided into two equal parts, which passed, in opposite directions, through the two wires of a very delicate differential-galvanometer. The wire to be studied was intercalated in one of the

circuits, in the other a length of the wire of a rheostat, of which the resistance was equivalent when the needle was at zero. The resistances R, R', ... of different wires submitted to experiment were proportional to the lengths l, l', ... of rheostat-wire which had served to measure them ; and in order to express them as functions of a given unit, it was sufficient if the ratio of the rheostat-wire itself to this unit had been determined once for all. The rheostat consisted simply of two identical, very regular platinum wires, stretched parallel on a horizontal rule of two metres length. These wires traversed a cork cup containing mercury, carried by a cursor movable along the rule. The current, arriving through the first wire, traversed the mercury and issued through the second wire. On the rule was a scale of millimetres ; and shifting the cursor n divisions increased or diminished the length of the circuit the value of $2n$. I shall not dwell upon the details, which permitted great precision to be attained, nor on the verifications which I made of the method and of the apparatus.

The wire under examination was soldered at each end to a copper rod, then wrapped round a cylinder of pipe-clay, and, finally, heated in a narrow and deep muffle which occupied the axis of a large wrought-iron jar. This was placed in a gas-stove with two concentric envelopes ; by introducing a suitable volatilizable substance, and heating to ebullition, the whole apparatus, and consequently the wire, was brought to a fixed and known temperature.

By determining thus the resistances of one and the same metal at various known temperatures, a number of points are obtained, from which the *curve of the resistances* can be constructed and its elements calculated.

The following are the temperatures which served for my determinations :—

Ebullition of water . 100° Ebullition of sulphur . 440°
       „   mercury 360   „   cadmium   860

I made, besides, a great number of measurements below 360°, the apparatus being filled with mercury and heated by a regular current of gas ; the temperature was indicated by thermometers placed in the muffle at different depths.

The results obtained by the preceding method were controlled and confirmed by determinations with Wheatstone's bridge, with the aid of a set of resistances similar to those employed in telegraphy.

The results are summed up in the Tables which follow.

In the first the conductivities at zero are expressed as functions of the two units which are now-a-days usually employed :—the theoretic absolute unit, or *ohm*, proposed by the British Association ; and the mercury unit, adopted by M. Werner Siemens. The third column gives the ratios of the conductivities to that of silver, in order that the results may be compared with the well-known coefficients of MM. Becquerel, Lenz, Matthiessen, &c.

The second Table gives the formulæ of the increment of the resistance with the temperature. This takes place regularly up to

the melting-point, following the ordinates of a curve the abscissæ of which represent the corresponding temperatures, and which generally differs very little from a right line. Comparing the resistances to the resistance at zero, they can be expressed by a formula of the form

$$R_t = R_0(1 + at + bt^2).$$

The constants a and b were calculated by the method of least squares, which makes all the observations cooperate for the determination of the most probable values of the unknown quantities.

The increment varies from one metal to another. In steel and in iron the initial resistance is doubled at about 170°; in silver, copper, and gold, at about 255°; in platinum, at about 455°. In alloys the increment is in general less: in German silver, for example, at 860° the resistance has increased by only 0·3 of its value at zero. The numbers in this Table express the variation of the *specific resistance*—*i. e.* of the resistance reduced in each case to the unit of length and section. If we wish to use them to calculate the resistance at $t°$ of *a given wire* the resistance of which at zero is known, account must be taken of the changes of dimension of the wire; in other words, the resistance obtained must be multiplied by $\dfrac{1}{1 + \delta t}$, δ being the coefficient of dilatation. This correction cannot be neglected when the temperature exceeds certain limits.

Specific Resistances of Metals at Zero.

	Resistance of 1 metre length and 1 square millimetre section.		Conductivities compared with silver.
	ohm.	Siemens unit.	
Pure silver, annealed	0·0154	0·0161	100
Copper, annealed..........	0·0171	0·0179	90
Silver ($\frac{1.5.0}{1000}$), annealed	0·0193	0·0201	80
Pure gold, annealed	0·0217	0·0227	71
Aluminium, annealed	0·0309	0·0324	49·7
Magnesium, cold-beaten	0·0423	0·0443	36·4
Pure zinc, annealed at 350°	0·0565	0·0591	27·5
Pure zinc, cold-beaten......	0·0594	0·0621	25·9
Pure cadmium, cold-beaten .	0·0685	0·0716	22·5
Brass, annealed	0·0691	0·0723	22·3
Steel, tempered	0·1099	0·1149	14·0
Pure tin	0·1161	0·1214	13·3
Aluminium bronze	0·1189	0·1243	13·0
Iron, tempered............	0·1216	0·1272	12·7
Palladium, tempered	0·1384	0·1447	11·1
Platinum, tempered........	0·1575	0·1647	9·77
Thallium	0·1831	0·1914	8·41
Pure lead................	0·1985	0·2075	7·76
German silver	0·2654	0·2755	5·80
Pure mercury	0·9564	1·0000	1·61

Variation of Resistance with Temperature.

Steel.............	$R_t = R_0$	$(1 + 0\cdot004978t + 0\cdot000007351t^2)$
Iron	,,	$(1 + 0\cdot004516t + 0\cdot0000005828t^2)$
Tin	,,	$(1 + 0\cdot0040028t + 0\cdot000005826t^2)$
Thallium	,,	$(1 + 0\cdot004125t + 0\cdot0000003488t^2)$
Cadmium	,,	$(1 + 0\cdot0042064t + 0\cdot000001765t^2)$
Zinc	,,	$(1 + 0\cdot004192t + 0\cdot000001481t^2)$
Lead	,,	$(1 + 0\cdot0039454t + 0\cdot000001430t^2)$
Aluminium	,,	$(1 + 0\cdot003876t + 0\cdot000001320t^2)$
Silver	,,	$(1 + 0\cdot003972t + 0\cdot000000687t^2)$
Magnesium	,,	$(1 + 0\cdot003870t + 0\cdot000000863t^2)$
Copper..........	,,	$(1 + 0\cdot003637t + 0\cdot000000587t^2)$
Gold	,,	$(1 + 0\cdot003678t + 0\cdot000000426t^2)$
Silver $(\frac{750}{1000})$	,,	$(1 - 0\cdot003522t + 0\cdot000000667t^2)$
Palladium	,,	$(1 + 0\cdot002787t + 0\cdot000000611t^2)$
Platinum	,,	$(1 + 0\cdot002454t + 0\cdot000000594t^2)$
Brass	,,	$(1 + 0\cdot001599t)$
Aluminium bronze	,,	$(1 + 0\cdot001020t)$
German silver....	,,	$(1 + 0\cdot000356t)$
Mercury.........	,,	$(1 + 0\cdot000882t + 0\cdot00000114t^2)$.

—*Comptes Rendus de l'Académie des Sciences*, vol. lxxvi. pp. 342–346.

ON THE CONDITIONS REQUISITE FOR THE MAXIMUM OF RESISTANCE OF GALVANOMETERS. BY M. TH. DU MONCEL.

Mr. Schwendler, and several other physicists previously, have found that, for a galvanometer to be in the best possible conditions of sensitiveness in relation to a circuit of given resistance, the resistance of its magnetizing helix must be equal to that of the exterior circuit in communication with it. Several experiments having demonstrated to me that the sensibility increases with the length of the helix-wire, under other conditions than those thus indicated, I submitted to calculation the galvanometric effects with regard to a circuit of given resistance; and I have ascertained that those conditions of sensitiveness demand a considerably greater length of wire in the multiplier than that which corresponds to the resistance of the exterior circuit.

To demonstrate the law which he had laid down, Schwendler endeavours to calculate the number t of the turns of the helix of the multiplier as a function of the space C occupied by the helix-wire, and also as a function of the resistance H of the latter. Designating by s the section of this wire, t becomes equal to $\frac{C}{s}$, and the length H of the helix equal to $\frac{Ct}{s}$; which supposes wrongly that the resistance is proportional to the number of the spiral turns, and inversely as the section of the wire.

According to these data, it would result from the combination of

the values of t and H that t would be expressed by $\sqrt{H}$; and as, besides, the value of the intensity of the current is $\dfrac{E}{R+H}$ (R being the resistance of the exterior circuit, E the electromotive force of the source of electricity), the magnetic moment F of the needle would be

$$F = \frac{E\sqrt{H}}{R+H},$$

an expression susceptible of a maximum for $R = H$.

But in reality the value of t is far from being expressed by $\sqrt{H}$; and if we amend the preceding formula by inserting the true quantities, we arrive at altogether different conditions.

In fact, let a be the thickness of the layers of spires, b the width of the galvanometric framework, c the diameter of the circular part which terminates it on each side, d the length of the framework between these two circular portions, and g the diameter of the wire (including its insulating covering); the number of the turns t will be expressed by $\dfrac{ab}{g^2}$, and the length H of the helix by

$$H = \frac{ab}{g^2}[(a+c)\pi + 2d];$$

consequently the magnetic moment F of the needle will be

$$F = \frac{\alpha\,E ab}{g^2 R + ab[(a+c)\pi + 2d]},$$

α denoting the constant of the instrument. Now the conditions of a maximum for this formula, taking a for variable, which is the sole quantity proportional to t, lead to the relation

$$R = \frac{\pi b a^2}{g^2},$$

which shows that the resistance of the galvanometer-wire must be greater than that of the exterior circuit by a quantity represented by

$$\frac{ab}{g^2}(\pi c + 2d).$$

We can, moreover, easily assure ourselves of the truth of this deduction by supposing the framework of the galvanometer represented by a simple coil, as in the Thomson galvanometer, and taking the magnetic moments of the needle with the two lengths of the wire of the helix corresponding to the two conditions for a maximum given by Mr. Schwendler and me. For greater simplicity, we will represent the diameter c by $2r$. In these conditions the length H of the helix, instead of being greater than the resistance R by a quantity represented by $\dfrac{ab}{g^2}(\pi c + 2d)$, will be greater only by a quantity $\dfrac{ab}{g^2}\pi c$, or will be, in proportion to R, as $a + 2r$ is to a. We shall therefore have :—

1. With $H = R$,

$$F = \frac{Eab}{2\pi ba(a+2r)};$$

2. With $H = R\frac{a+2r}{a}$,

$$F' = \frac{Eab}{2\pi ba'(a'+r)},$$

which leads to

$$\frac{F'}{F} = \frac{a+2r}{a'+r}.$$

And as then we have $\dfrac{\pi ba(a+2r)}{g^2} = \dfrac{\pi ba'}{g^2}$, we deduce $a' = \sqrt{a(a+2r)}$, and consequently

$$\frac{F'}{F} = \frac{a+2r}{\sqrt{a^2+2ar+r^2}}.$$

As $a + 2r$ can be put in the form $\sqrt{a^2+2ar+r^2} + r$, it is at once seen that F' is greater than F.

The experimental verification of the above-exhibited deduction not being easy to realize, on account of the too great sensitiveness of galvanometers with resistant helices and with continual variations of the resistance of the exterior circuit with galvanometers of short helix, I made the experiment with electromagnets, the attractive force of which, reckoned according to the laws of MM. Dub and Jacobi, admits of the same conditions of a maximum relative to the resistance of the magnetizing helices, as I have shown in my researches on the best construction of electromagnets. Now the following are the results I obtained with one and the same electromagnet excited by a Daniell pile of 20 elements, to which I applied successively magnetizing coils of two different resistances, viz. a resistance of 75 kilometres of telegraphic wire of 4 millims. diameter, and a resistance of 200 kilometres. In order to avoid static reactions, the attractive forces were measured with 1 millimetre distance of separation from the armature.

(1) Forces of the electromagnet with coils of 75 kilometres resistance.

			metres.	metres.	Attractive force. grammes.
The exterior circuit having			18620 +	0	80
,,	,,	,,	18620 +	100000	15
,,	,,	,,	18620 +	200000	5·5
,,	,,	,,	18620 +	370000	0

(2) Forces of the electromagnet with coils of 200 kilometres resistance.

			metres.	metres.	Attractive force. grammes.
The exterior circuit having			18620 +	0	58
,,	,,	,,	18620 +	100000	25
,,	,,	,,	18620 +	200000	14
,,	,,	,,	18620 +	370000	0

The wire of these circuits was perfectly insulated; and the time

during which the current was closed was long enough to develop the maximum of magnetization.

Now we see by these numbers that it is when the resistance of the coils is much superior to the exterior resistance that the maximum is attained, and that, *for the exterior circuit of* 100 *kilometres, the maximum is obtained with the helices of* 200 *kilometres*—that is to say, helices presenting double the resistance. Calculation leads to the same deduction; for in the electromagnet the thickness a of the helix was equal to the diameter c of the electromagnet, and consequently the value of H, which must be equal to $R\dfrac{a+c}{a}$ to satisfy the conditions of a maximum, was found, under the circumstances of my experiments, to be equal to 2R.—*Comptes Rendus de l'Acad. des Sciences,* vol. lxxvi. pp. 368–371.

STRATIFICATION IN A LIQUID IN OSCILLATORY MOTION.
BY J. STEFAN.

Fine powder diffused in a horizontal tube containing a column of water set in motion to and fro distributes itself in slices or strata perpendicular to the direction of the motion. To study this, M. Stefan employed a very simple arrangement, which consists in introducing water holding in suspension oxide of iron into a horizontal tube of glass with two vertical prolongations. To one of these vertical branches is fitted a piece of caoutchouc tubing closed at its free extremity with a cork. By pressing this between the finger and thumb an impulse is communicated to the column of water; and by repeating the pressure at equal intervals a regular reciprocating motion is impressed upon it. The powder which has sunk to the lower part of the glass tube is then distributed in streaks which are so much finer and closer as the motion of the water which carries them with it has less amplitude.

M. Stefan explains this grouping of the particles by the fact that their displacement is effected more easily in one direction than in the other—which depends either upon the water moving less quickly in one of the two phases of its oscillatory motion than in the other, or else on the shape of the particles. We have here a fact analogous to that observed in the production of the lycopodium figures obtained by M. Kundt in acoustic tubes, and having also some affinity to the stratification of the electric light. At least M. Stefan thinks that this latter phenomenon may be attributed to a cause altogether similar to the one just indicated. " In the Geissler tubes," he says, " a part of the gaseous molecules, playing the same part as the powder-particles in the above experiment, receive from the alternate discharges impulses which are more powerful in one direction than in the other. As to the influence which may be exerted by the particular nature of the molecules, it is not necessary to attribute it to their form departing more or less from the spherical; it is sufficient if we invoke the variety of the motions produced by heat in the different molecules at a given moment."— *Archives des Sciences Physiques et Naturelles,* vol. xlvi. p. 270.

XLI. *On the Ultramundane Corpuscules of* Le Sage, *also on the Motion of Rigid Solids in a Liquid circulating irrotationally through perforations in them or in a Fixed Solid.* By Sir WILLIAM THOMSON, *F.R.S.**

LE SAGE, born at Geneva in 1724, devoted the last sixty-three years of a life of eighty to the investigation of a mechanical theory of gravitation. The probable existence of a gravific mechanism is admitted, and the importance of the object to which Le Sage devoted his life pointed out, by Newton and Rumford† in the following statements :—

"It is inconceivable that inanimate brute matter should, without the mediation of something else, which is not material, operate upon, and affect other matter without mutual contact; as it must do, if gravitation, in the sense of *Epicurus*, be essential and inherent in it. And this is the reason why I desired you would

* Communicated by the Author, from the Proceedings of the Royal Society of Edinburgh, 1871–72.

† On the other hand, by the middle of last century the mathematical naturalists of the Continent, after half a century of resistance to the Newtonian principles (which, both by them and by the English followers of Newton, were commonly supposed to mean the recognition of gravity as a force acting simply at a distance without mediation of intervening matter), had begun to become more " Newtonian " than Newton himself. On the 4th February, 1744, Daniel Bernoulli wrote as follows to Euler : " Uebrigens glaube ich, dass der Aether sowohl *gravis versus solem*, als die Luft versus terram sey, und kann Ihnen nicht bergen, dass ich über diese Puncte ein völliger Newtonianer bin, und verwundere ich mich, dass Sie den Principiis Cartesianis so lang adhäriren ; es möchte wohl einige Passion vielleicht mit unterlaufen. Hat Gott können eine *animam*, deren Natur uns unbegreiflich ist, erschaffen, so hat er auch können eine attractionem universalem materiæ imprimiren, wenn gleich solche attractio *supra captum* ist, da hingegen die Principia Cartesiana allzeit *contra captum* etwas involviren."

not ascribe innate gravity to me. That gravity should be innate, inherent, and essential to matter, so that one body may act upon another at a distance through a *vacuum*, without the mediation of any thing else, by and through which their action and force may be conveyed from one to another, is to me so great an absurdity, that I believe no man who has in philosophical matters a competent faculty of thinking, can ever fall into it. Gravity must be caused by an agent acting constantly according to certain laws; but whether this agent be material or immaterial, I have left to the consideration of my readers."—NEWTON's *Third Letter to Bentley*, February 25th, 1692–3.

" Nobody surely, in his sober senses, has ever pretended to understand the mechanism of gravitation; and yet what sublime discoveries was our immortal Newton enabled to make, merely by the investigation of the laws of its action " *.

Le Sage expounds his theory of gravitation, so far as he had advanced it up to the year 1782, in a paper published in the Transactions of the Royal Berlin Academy for that year, under the title " Lucrèce Newtonien." His opening paragraph, entitled " But de ce mémoire," is as follows :—

" Je me propose de faire voir : que si les premiers Epicuriens avoient eu ; sur la Cosmographie des idées aussi saines seulement, que plusieurs de leurs contemporains, qu'ils négligeoient d'écouter † ; et sur la Géométrie, une partie des connoissances qui étoient déjà communes alors : ils auroient, tres probablement, découvert sans effort ; les Loix de la Gravité universelle, et sa Cause mécanique. *Loix* ; dont l'invention et la démonstration, font la plus grande gloire du plus puissant génie qui ait jamais existé : et *Cause*, qui après avoir fait pendant longtems, l'ambition des plus grands Physiciens ; fait à présent, le désespoir de leurs successeurs. De sorte que, par exemple, les fameuses Règles de *Kepler* ; trouvées il y a moins de deux siècles, en partie sur des conjectures gratuites, et en partie après d'immenses tâtonnemens ; n'auroient été que des corollaires particuliers et inévitables, des lumières générales que ces anciens Philosophes pouvoient puiser (comme en se jouant) dans le mécanisme proprement dit de la Nature. Conclusion ; qu'on peut appliquer exactement aussi, aux Loix de *Galilée* sur la chûte des Graves sublunaires ; dont la découverte a été plus tardive encore, et plus contestée : joint à ce que, les expériences sur lesquelles cette découverte étoit établie ; laissoient dans leurs résultats (nécessairement grossiers),

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* " An Inquiry concerning the Source of the Heat which is excited by Friction. By Count Rumford," Philosophical Transactions, 1798.

† " Vobis (Epicureis) minùs notum est, quemadmodum quidque dicatur. Vestra enim solùm legitis, vestra amatis ; cæteros, causâ incognitâ, condemnatis.- CICÉRON, *De natura Deorum*, ii. 29."

une latitude, que les rendoit également compatibles avec plusieurs autres hypothèses ; qu'aussi, l'on ne manqua pas de lui opposer : au lieu que, les conséquences du choc des Atoms ; auroient été absolument univoques en faveur du seul principe véritable (des Accélérations égales en Tempuscules égaux)."

If Le Sage had but excepted Kepler's third law, it must be admitted that his case, as stated above, would have been thoroughly established by the arguments of his " mémoire ; " for the Epicurean assumption of parallelism adopted to suit the false idea of the earth being flat, prevented the discovery of the law of the inverse square of the distance, which the mathematicians of that day were quite competent to make, if the hypothesis of atoms moving in all directions through space, and rarely coming into collision with one another, had been set before them, with the problem of determining the force with which the impacts would press together two spherical bodies, such as the earth and moon were held to be by some of the contemporary philosophers to whom the Epicureans " would not listen." But nothing less than direct observation, proving Kepler's third law—Galileo's experiment on bodies falling from the tower of Pisa, Boyle's guinea-and-feather experiment, and Newton's experiment of the vibrations of pendulums composed of different kinds of substance —could either give the idea that gravity is proportional to mass, or prove that it is so to a high degree of accuracy for large bodies and small bodies, and for bodies of different kinds of substance. Le Sage sums up his theory in an appendix to the " Lucrèce Newtonien," part of which, translated (literally, except a few sentences which I have paraphrased), is as follows :—

Constitution of Heavy Bodies.

1st. Their indivisible particles are cages—for example, empty cubes or octahedrons vacant of matter except along the twelve edges.

2nd. The diameters of the bars of these cages, supposed increased each by an amount equal to the diameter of one of the gravific corpuscles, are so small relatively to the mutual distance of the parallel bars of each cage, that the terrestrial globe does not intercept even so much as a ten-thousandth part of the corpuscles which offer to traverse it.

3rd. These diameters are all equal ; or if they are unequal, their inequalities sensibly compensate one another [in averages].

Constitution of Gravific Corpuscles.

1st. Conformably to the second of the preceding suppositions, their diameters added to that of the bars is so small relatively to

Y 2

the mutual distance of parallel bars of one of the cages, that the weights of the celestial bodies do not differ sensibly from being in proportion to their masses.

2nd. They are isolated ; so that their progressive movements are necessarily linear.

3rd. They are so sparsely distributed (that is to say, their diameters are so small relatively to their mean mutual distances) that not more than one out of every hundred of them meets another corpuscule during several thousands of years ; so that the uniformity of their movements is scarcely ever troubled sensibly.

4th. They move along several hundred thousand millions of different directions, in counting for one same direction all those which are [within a definite very small angle of being] parallel to one straight line.   The distribution of these straight lines is to be conceived by imagining as many points as one wishes to consider of different directions, scattered over a globe as uniformly as possible, and therefore separated from one another by at least a second of angle, and then imagining a radius of the globe drawn to each of those points.

5th. Parallel, then, to each of those directions, let a current or torrent of corpuscules move ; but, not to give the stream a greater breadth than is necessary, consider the transverse section of this current to have the same boundary as the orthogonal projection of the visible world on the plane of the section.

6th. The different parts of one such current are sensibly equidense, whether we compare, among one another, collateral portions of sensible transverse dimensions, or successive portions of such lengths that their times of passage across a given surface are sensible.   And the same is to be said of the different currents compared with one another.

7th. The mean velocities, defined in the same manner as I have just defined the densities, are also sensibly equal.

8th. The ratios of these velocities to those of the planets are several million times greater than the ratios of the gravities of the planets towards the sun to the greatest resistance which secular observations allow us to suppose they experience.   For example, [these velocities must be] some hundredfold a greater number of times the velocity of the earth, than the ratio of 190,000 * times the gravity of the earth towards the sun to the greatest resistance which secular observations of the length of the year permit us to suppose that the earth experiences from the celestial masses.

* To render the sentence more easily read, I have substituded this number in place of the following words :—" le nombre de fois que le firmament contient le disque apparent du soleil."

Conception, *which facilitates the application of Mathematics to determine the mutual Influence of these Heavy Bodies, and these Corpuscules.*

1st. Decompose all heavy bodies into molecules of equal mass, so small that they may be treated as attractive points with respect to theories in which gravity is considered without reference to its cause; that is to say, each must be so small that inequalities of distance and differences of direction between its particles and those of another molecule, conceived as attracting it and being attracted by it, may be neglected. For example, suppose the diameter of the molecule considered to be a hundred thousand times smaller than the distance between two bodies of which the mutual gravitation is examined, which would make its apparent semidiameter, as seen from the other body, about one second of angle.

2nd. For the surfaces of such a molecule, accessible but impermeable to the gravific fluid, substitute one single spherical surface equal to their sum.

3rd. Divide those surfaces into facets small enough to allow them to be treated as planes, without sensible error [&c. &c.].

Remarks.

It is not necessary to be very skilful to deduce from these suppositions all the laws of gravity, both sublunary and universal (and consequently also those of Kepler, &c.), with all the accuracy with which observed phenomena have proved those laws. Those laws, therefore, are inevitable consequences of the supposed constitutions.

2nd. Although I here present these constitutions crudely and without proof, as if they were gratuitous hypotheses and hazarded fictions, equitable readers will understand that on my own part I have at least some presumptions in their favour (independent of their perfect agreement with so many phenomena), but that the development of my reasons would be too long to find a place in the present statement, which may be regarded as a publication of theorems without their demonstrations.

3rd. . . . . . There are details upon which I have wished to enter on account of the novelty of the doctrine, and which will readily be supplied by those who study it in a favourable and attentive spirit. If the authors who write on hydrodynamics, aërostatics, or optics had to deal with captious readers, doubting the very existence of water, or air, or light, and therefore not adapting themselves to any tacit supposition regarding equivalencies or compensations not expressly mentioned in their treatises, they would be obliged to load their definitions with a

vast number of specifications which instructed or indulgent readers do no not require of them.   One understands "*à demi-mot*" and "*sano sensu*" only familiar propositions towards which one is already favourably inclined.

Some of the details referred to in this concluding sentence of the appendix to his *Lucrèce Newtonien*, Le Sage discusses fully in his *Traité de Physique Mécanique*, edited by Pierre Prévost, and published in 1818 (Geneva and Paris).

This treatise is divided into four books.

I. " Exposition sommaire du système des corpuscules ultra-mondains."

II. " Discussion des objections qui peuvent s'élever contre le système des corpuscules ultramondains."

III. " Des fluids élastiques ou expansifs."

IV. " Application des théories précédentes à certaines affi-nités."

It is in the first two books that gravity is explained by the impulse of ultramundane corpuscules, and I have no remarks at present to make on the third and fourth books.

From Le Sage's fundamental assumptions, given above as nearly as may be in his own words, it is, as he says himself, easy to deduce the law of the inverse square of the distance, and the law of proportionality of gravity to mass.   The object of the present note is not to give an exposition of Le Sage's theory, which is sufficiently set forth in the preceding extracts, and discussed in detail in the first two books of his posthumous treatise. I may merely say that inasmuch as the law of the inverse square of the distance, for every distance, however great, would be a perfectly obvious consequence of the assumptions, were the gravific corpuscules infinitely small, and therefore incapable of coming into collision with one another, it may be extended to as great distances as we please, by giving small enough dimensions to the corpuscules relatively to the mean distance of each from its nearest neighbour.   The law of masses may be extended to as great masses as those for which observation proves it (for example, the mass of Jupiter), by making the diameters of the bars of the supposed cage-atoms constituting heavy bodies, small enough.   Thus, for example, there is nothing to prevent us from supposing that not more than one straight line of a million drawn at random towards Jupiter and continued through it, should touch one of the bars.   Lastly, as Le Sage proves, the resistance of his gravific fluid to the motion of one of the planets through it, is proportional to the product of the velocity of the planet into the average velocity of the gravific corpuscules ; and hence, by making the velocities of the corpuscules great enough,

and giving them suitably small masses, they may produce the actual forces of gravitation, and not more than the amount of resistance which observation allows us to suppose that the planets experience. It will be a very interesting subject to examine minutely Le Sage's details on these points, and to judge whether or not the additional knowledge gained by observation since his time requires any modification to be made in the estimate which he has given of the possible degrees of permeability of the sun and planets, of the possible proportions of diameters of corpuscules to interstices between them in the " gravific fluid," and of the possible velocities of its component corpuscules. This much is certain, that if hard indivisible atoms are granted at all, his principles are unassailable, and nothing can be said against the probability of his assumptions. The only imperfection of his theory is that which is inherent to every supposition of hard, indivisible atoms. They must be perfectly elastic or imperfectly elastic, or perfectly inelastic. Even Newton seems to have admitted as a probable reality hard, indivisible, unalterable atoms, each perfectly inelastic.

Nicolas Fatio is quoted by Le Sage and Prévost as a friend of Newton, who in 1689 or 1690 had invented a theory of gravity perfectly similar to that of Le Sage, except certain essential points, had described it in a Latin poem not yet printed, and had written, on the 30th March, 1694, a letter regarding it, which is to be found in the third volume of the works of Leibnitz, having been communicated for publication to the editor of those works by Le Sage. Redeker, a German physician, is quoted by Le Sage as having expounded a theory of gravity of the same general character, in a Latin dissertation published in 1736, referring to which Prévost says, " Où l'on trouve l'exposé d'un système fort semblable à celui de Le Sage dans ses traits principaux, mais dépourvu de cette analyse exacte des phénomènes qui fait le principal mérite de toute espèce de théorie." Fatio supposed the corpuscules to be elastic, and seems to have shown no reason why their return velocities after collision with mundane matter should be less than their previous velocities, and therefore not to have explained gravity at all. Redeker, we are told by Prévost, had very limited ideas of the permeabilities of great bodies, and therefore failed to explain the law of the proportionality of gravity to mass : " he enunciated this law very correctly in section 15 of his dissertation ; but the manner in which he explains it shows that he had but little reflected upon it. Notwithstanding these imperfections, one cannot but recognize in this work an ingenious conception which ought to have provoked examination on the part of naturalists, of whom many at that time occupied themselves with the same investigation.

Indeed there exists a dissertation by Segner on this subject[*].
But science took another course, and works of this nature gradu-
ally lost appreciation.   Le Sage has never failed on any occasion
to call attention to the system of Redeker, as also to that of
Fatio " [†].

Le Sage shows that, to produce gravitation, those of the ultra-
mundane corpuscules which strike the cage-bars of heavy bodies
must either stick there or go away with diminished velocities.
He supposed the corpuscules to be inelastic (*durs*), and points out
that we ought not to suppose them to be permanently lodged in
the heavy body (*entassés*), that we must rather suppose them to
slip off, but that, being inelastic, their average velocities after
collision must be less than that which they had before collision[‡].

That these suppositions imply a gradual diminution of gravity
from age to age was carefully pointed out by Le Sage, and re-
ferred to as an objection to his theory. Thus he says, " . . . Donc,
la durée de la gravité seroit *finie* aussi, et par conséquent la durée
du monde.

Réponse.   Concedo ; mais pourvu que cet obstacle ne contribue
pas à faire finir le monde plus promptement qu'il n'auroit fini
sans lui, il doit être considéré comme nul " [§].

Two suppositions may be made on the general basis of Le
Sage's doctrine :—

1st (which seems to have been Le Sage's belief).   Suppose
the whole of mundane matter to be contained within a finite space,
and the infinite space round it to be traversed by ultramundane
corpuscules, and a small proportion of the corpuscules coming
from ultramundane space to suffer collisions with mundane mattter,
and get away with diminished gravific energy to ultramundane
space again.   They would never return to the world were it not
for collision among themselves and other corpuscules.   Le Sage
held :—that such collisions are extremely rare that ; each collision,
even between the ultramundane corpuscules themselves, destroys
some energy ‖; that at a not infinitely remote past time they were
set in motion for the purpose of keeping gravitation throughout
the world in action for a limited period of time ; and that both by

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[*] De Causâ gravitatis Redekerianâ.

[†] Le Sage was remarkably scrupulous in giving full information regar-
ding all who preceded him in the development of any part of his theory.

[‡] Le Sage estimated the velocity after collision to be two thirds of the
velocity before collision.

[§] Posthumous *Traité de Physique Mécanique*, edited by Pierre Prévost.
Geneva and Paris, 1818.

[‖] Newton (Optics, Query 30, ed. 1721, p. 373) held that two equal
and similar atoms, moving with equal velocities in contrary directions,
come to rest when they strike one another.   Le Sage held the same ; and
it seems that writers of last century understood this without qualification
when they called atoms hard.

their mutual collisions, and by collisions with mundane atoms, the whole stock of gravific energy is being gradually reduced, and therefore the intensity of gravity gradually diminishing from age to age.

Or, 2nd, suppose mundane matter to be spread through all space, but to be much denser within each of an infinitely great number of finite volumes (such as the volume of the earth) than elsewhere. On this supposition, even were there no collisions between the corpuscules themselves, there would be a gradual diminution in their gravific energy through the repeated collisions with mundane matter which each one must in the course of time suffer. The secular diminution of gravity would be more rapid according to this supposition than according to the former, but still might be made as slow as we please by pushing far enough the fundamental assumptions of very small diameters for the cage-bars of the mundane atoms, very great density for their substance, and very small volume and mass, and very great velocity for the ultramundane corpuscules.

The object of the present note is to remark that (even although we were to admit a gradual fading away of gravity, if slow enough), we are forbidden by the modern physical theory of the conservation of energy to assume inelasticity, or any thing short of perfect elasticity, in the ultimate molecules, whether of ultramundane or of mundane matter—and, at the same time, to point out that the assumption of diminished exit-velocity of ultramundane corpuscules, essential to Le Sage's theory, may be explained for perfectly elastic atoms, consistently both with modern thermodynamics, and with perennial gravity.

If the gravific corpuscules leave the earth or Jupiter with less energy than they had before collision, their effect must be to continually elevate the temperature throughout the whole mass. The energy which must be attributed to the gravific corpuscules is so enormously great, that this elevation of temperature would be sufficient to melt and evaporate any solid, great or small, in a fraction of a second of time. Hence, though outward-bound corpuscules must travel with less velocity, they must carry away the same energy with them as they brought. Suppose, now, the whole energy of the corpuscules approaching a planet to consist of translatory motion : a portion of the energy of each corpuscule which has suffered collision must be supposed to be converted by the collision into vibrations, or vibrations and rotations. To simplify ideas, suppose for a moment the particles to be perfectly smooth elastic globules. Then collision could not generate any rotatory motion ; but if the cage-atoms constituting mundane matter be each of them, as we must suppose it to be, of enormously great mass in comparison with one of the ultramundane globules, and if the substance of the latter, though perfectly

elastic, be much less rigid than that of the former, each globule that strikes one of the cage-bars must (Thomson and Tait's 'Natural Philosophy,' § 301) come away with diminished velocity of translation, but with the corresponding deficiency of energy altogether converted into vibration of its own mass. Thus the condition required by Le Sage's theory is fulfilled without violating modern thermo-dynamics; and, according to Le Sage, we might be satisfied not to inquire what becomes of those ultramundane corpuscules which have been in collision either with the cage-bars of mundane matter or with one another; for at present, and during ages to come, these would be merely an inconsiderable minority, the great majority being still fresh with original gravific energy unimpaired by collision. Without entering on the purely metaphysical question, Is any such supposition satisfactory? I wish to point out how gravific energy may be naturally restored to corpuscules in which it has been impaired by collision.

Clausius has introduced into the kinetic theory of gases the very important consideration of vibrational and rotational energy. He has shown that a multitude of elastic corpuscules moving through void, and occasionally striking one another, must, on the average, have a constant proportion of their whole energy in the form of vibrations and rotations, the other part being purely translational. Even for the simplest case—that, namely, of smooth elastic globes—no one has yet calculated by abstract dynamics the ultimate average ratio of the vibrational and rotational to the translational energy. But Clausius has shown how to deduce it for the corpuscules of any particular gas from the experimental determination of the ratio of its specific heat, pressure constant, to its specific heat, volume constant[*]. He found that

$$\beta = \frac{2}{3}\frac{1}{\gamma-1},$$

if γ be the ratio of the specific heats, and β the ratio of the whole energy to the translational part of it. For air, the value of γ found by experiment is $1\cdot408$, which makes $\beta=1\cdot634$. For steam, Maxwell says, on the authority of Rankine, β "may be as much as $2\cdot19$; but this is very uncertain." If the molecules of gases are admitted to be elastic corpuscules, the validity of Clausius's principle is undeniable; and it is obvious that the value of the ratio β must depend upon the shape of each molecule, and on the distribution of elastic rigidity through it, if its substance is not homogeneous. Further, it is clear that the value of β, for a set of equal and similar corpuscules, will not be

* Maxwell's 'Elementary Treatise on Heat,' chap. xxii. Longmans, 1871.

the same after collision with molecules different from them in form or in elastic rigidity as after collision with molecules only of their own kind. All that is necessary to complete Le Sage's theory of gravity in accordance with modern science, is to assume that the ratio of the whole energy of the corpuscules to the translational part of their energy is greater, on the average, after collisions with mundane matter than after intercollisions of only ultramundane corpuscules. This supposition is neither more nor less questionable than that of Clausius for gases, which is now admitted as one of the generally recognized truths of science. The corpuscular theory of gravity is no more difficult in allowance of its fundamental assumptions than the kinetic theory of gases as at present received; and it is more complete, inasmuch as, from fundamental assumptions of an extremely simple character, it explains all the known phenomena of its subject, which cannot be said of the kinetic theory of gases so far as it has hitherto advanced.

Postscript, April 1872.

In the preceding statement I inadvertently omitted to remark that if the constituent atoms are æolotropic in respect of permeability, crystals would generally have different permeabilities in different directions, and would therefore have different weights according to the direction of their axes relatively to the direction of gravity. No such difference has been discovered; and it is certain that, if there is any, it is extremely small. Hence the constituent atoms, if æolotropic as to permeability, must be so but to an exceedingly small degree. Le Sage's second fundamental assumption, given above under the title " *Constitution of Heavy Bodies*," implies sensibly equal permeability in all directions, even in an æolotropic structure, unless much greater than Jupiter, provided that the atoms are isotropic as to permeability.

A body having different permeabilities in different directions would, if of manageable dimensions, give us a means for drawing energy from the inexhaustible stores laid up in the ultramundane corpuscules, thus :—First, turn the body into a position of minimum weight; secondly, lift it through any height; thirdly, turn it into a position of maximum weight; fourthly, let it down to its primitive level. It is easily seen that the first and third of those operations are performed without the expenditure of work; and, on the whole, work is done by gravity in operations 2 and 4. In the corresponding set of operations performed upon a movable body in the neighbourhood of a fixed magnet, as much work is required for operations 1 and 3 as is gained in operations 2 and 4—the magnetization of the movable body being either intrinsic or inductive, or partly intrinsic and partly

inductive, and the part of its æolotropy (if any) which depends
on inductive magnetization being due either to magnecrystallic
quality of its substance or to its shape*.

On the Motion of Rigid Solids in a Liquid circulating irrotationally through perforations in them or in a Fixed Solid†.

1. Let ψ, ϕ, ... be the values at time t, of generalized coordinates fully specifying the positions of any number of solids
movable through space occupied by a perfect liquid destitute of
rotational motion, and not acted on by any force which could
produce it.   Some or all of these solids being perforated, let
χ, χ', χ'', &c. be the quantities of liquid which from any era of
reckoning, up to the time t, have traversed the several apertures.
According to an extension of Lagrange's general equations of
motion, used in vol. i. of Thomson and Tait's 'Natural Philosophy,' §§ 331–336, proved in §§ 329, 331 of the German
translation of that volume, and to be further developed in the
second English edition now in the press, we may use these quantities χ, χ', ... as if they were coordinates so far as concerns the
equations of motion.   Thus, although the position of any part
of the fluid is not only not explicitly specified, but is actually
indeterminate, when ψ, ϕ, ... χ, χ', ... are all given, we may
regard χ, χ' ... as specifying all that it is necessary for us to
take into account regarding the motion of the liquid, in forming
the equations of motion of the solids; so that if ξ, η, ..., and
Ψ, Φ ... denote the generalized components of momentum and
of force [Thomson and Tait, § 313 (*a*) (*b*)] relatively to ψ, ϕ...,
and if κ, κ', ... K, K' ... denote corresponding elements relatively to χ, χ', ..., we have (Hamiltonian form of Lagrange's
general equations)

$$\left.\begin{array}{ll} \dfrac{d\xi}{dt} + \dfrac{\eth \tau}{d\psi} = \Psi, & \dfrac{d\eta}{dt} + \dfrac{\eth T}{d\phi} = \Phi, \dots \\[2mm] \dfrac{d\kappa}{dt} + \dfrac{\eth T}{d\chi} = K, & \dfrac{d\kappa'}{dt} + \dfrac{\eth T}{d\chi'} = K', \dots \end{array}\right\} \quad \dots \quad (1)$$

where T denotes the whole kinetic energy of the system, and $\eth$
differentiation on the hypothesis of ξ, η, ... κ, κ' ... constant.

* "Theory of Magnetic Induction in crystalline and non-crystalline substances," Phil. Mag., March 1851; " Forces experienced by inductively
magnetized ferro-magnetic and diamagnetic non-crystalline substances,"
Phil. Mag., Oct. 1850; " Reciprocal action of diamagnetic particles,"
Phil. Mag., Dec. 1855; all to be found in a collection of reprinted and
newly written papers on electrostatics and magnetism, nearly ready for
publication (Macmillan, 1872).

† The title and first part (§§ 1–13) are new; the remainder (§§ 14, 15)
was communicated to the Royal Society at the end of last December.—
W. T., September 26, 1872.

2. To illustrate the meaning of χ, K, κ, χ', ..., let B be one of the perforated solids, to be regarded generally as movable, draw an immaterial barrier surface Ω across the aperture to which they are related, and consider this barrier fixed relatively to B. Let N denote the normal component velocity, relatively to B and Ω of the fluid at any point of Ω; and let $\iint d\sigma$ denote integration over the whole area of Ω; then

$$\iint N d\sigma = \dot{\chi}, \quad \ldots \ldots \ldots \quad (2)$$

and

$$\chi - \int \bar{dt} \iint N d\sigma, \quad \ldots \ldots \quad (3)$$

which is a symbolical expression of the definition of χ. To the surface of liquid coinciding with Ω at any instant, let pressure be applied of constant value K per unit of area, over the whole area; and at the same time let force (or force and couple) be applied to B equal and opposite to the resultant of this pressure supposed for a moment to act on a rigid material surface Ω rigidly connected with B. The "motive" (that is to say, system of forces) consisting of the pressure K on the fluid surface, and force and couple B as just defined, constitutes the generalized component force corresponding to χ [Thomson and Tait, § 313 (b)]; for it does no work upon any motion of B or other bodies of the system if χ is kept constant; and if χ varies, work is done at the rate

$$K\dot{\chi} \text{ per unit of time,}$$

whatever other motions or forces there may be in the system. Lastly, calling the density of the fluid unity, let κ denote "circulation"* [V. M. § 60 (a)]† of the fluid in any circuit crossing β once, and only once; it is this which constitutes the generalized component momentum relatively to χ [Thomson and Tait, § 313 (e)]; for (V. M. § 72) we have

$$\kappa = \int_0^t K dt, \quad \ldots \quad \ldots \quad (4)$$

if the system given at rest (or in any state of motion for which $\kappa = 0$) be acted on by the motive K during time $t\ddagger$.

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* Or $\int F ds$ if F denote the tangential component of the absolute velocity of the fluid at any point of the circuit, and $\int ds$ line integration once round the circuit.

† References distinguished by the initials V. M. are to the part already published of the author's paper on Vortex Motion (Transactions of the Royal Society of Edinburgh, 1867-8 and 1868-9).

‡ The general limitation, for impulsive action, that the displacements effected during it are infinitely small, is not necessary in this case. Compare § 5 (11) below.

3. The kinetic energy T is, of course, necessarily a quadratic function of the generalized momentum-components $\xi, \eta, \ldots \kappa, \kappa'\ldots$; with coefficients generally functions of $\psi, \phi, \ldots$, but necessarily independent of $\chi, \chi', \ldots$. In consequence of this peculiarity it is convenient to put

$$T = Q(\xi - \alpha\kappa - \alpha'\kappa' - \&c., \eta - \beta\kappa - \beta'\kappa' - \&c., \ldots) + \mathbb{Q}(\kappa, \kappa', \ldots), \quad (5)$$

where Q, $\mathbb{Q}$ denote two quadratic functions. This we may clearly do, because, if i be the number of the variables $\xi, \eta, \ldots$, and j the number of $\kappa, \kappa', \ldots$, the whole number of coefficients in the single quadratic function expressing τ is $\dfrac{(i+j)(i+j+1)}{2}$, which is equal to the whole number of the coefficients $\dfrac{i(i+1)}{2} + \dfrac{j(j+1)}{2}$ of the two quadratic functions, together with the i, j available quantities $\alpha, \beta, \ldots \alpha', \beta', \ldots, \ldots$.

4. The meaning of the quantities $\alpha, \beta, \ldots \alpha', \ldots$ thus introduced is evident when we remember that

$$\frac{dT}{d\xi} = \dot{\psi}, \quad \frac{dT}{d\eta} = \dot{\phi}, \ldots \quad \frac{dT}{d\kappa} = \dot{\chi}, \quad \frac{dT}{d\kappa'} = \dot{\chi}', \ldots \quad (6)$$

For, differentiating (5) and using these, we find

$$\dot{\psi} = \frac{dQ}{d\xi}, \quad \dot{\phi} = \frac{dQ}{d\eta}; \quad \ldots \quad (7)$$

and using these latter,

$$\dot{\chi} = \frac{d\mathbb{Q}}{d\kappa} - \alpha\dot{\psi} - \beta\dot{\phi} - \&c., \quad \dot{\chi}' = \frac{d\mathbb{Q}}{d\kappa'} - \alpha'\dot{\psi} - \beta'\dot{\phi} - \&c., \ldots \quad (8)$$

Equations (8) show that $-\alpha\dot{\psi}, -\beta\dot{\phi}, -\alpha'\dot{\psi}$, &c. are the contributions to the flux across Ω, Ω', &c. given by the separate velocity-components of the solids. And (7) show that to prevent the solids from being set in motion when impulses $\kappa, \kappa', \ldots$ are applied to the liquid at the barrier surfaces, we must apply to them impulses expressed by the equations

$$\xi = \alpha\kappa + \alpha'\kappa' + \&c., \quad \eta = \beta\kappa + \beta'\kappa' + \&c. \ldots \quad (9)$$

5. To form the equations of motion, we have in the first place

$$\frac{\eth T}{d\chi} = 0, \quad \frac{\eth T}{d\chi'} = 0, \ldots, \quad \ldots \quad (10)$$

and therefore, by (1),

$$\frac{d\kappa}{dt} = K, \quad \frac{d\kappa'}{dt} = K', \ldots, \quad \ldots \quad (11)$$

which show that the acceleration of κ, under the influence of $\mathbf{K}$, follows simply the law of acceleration of a mass under the influence of a force. Again (for the motions of the solids), let

$$\xi_0 = \xi - \alpha\kappa - \alpha'\kappa' - \&c., \quad \eta_0 = \eta - \beta\kappa - \beta'\kappa' - \&c., \ldots; \quad (12)$$

and let $\dfrac{\mathfrak{D}Q}{d\psi}$, &c. denote variations of Q on the hypothesis of ξ_0, $\eta_0, \ldots$ each constant.

We have from (5), remembering that $\dfrac{\eth T}{d\psi}$ &c. denote variations of T, on the hypothesis of $\xi, \eta, \ldots \kappa, \kappa', \ldots$ constant,

$$\left(\frac{\eth T}{d\psi} = \frac{\mathfrak{D}Q}{d\psi} - \frac{dQ}{d\xi} - d\kappa \frac{d\alpha}{d\psi} + \kappa' \frac{d\alpha'}{d\psi} + \ldots \right)$$
$$- \frac{dQ}{\eta}\left(\kappa \frac{d\beta}{d\psi} + \kappa' \frac{d\beta'}{d\psi} + \&c. \right) - \&c. + \frac{\eth Q}{d\psi},$$

or, by (7),

$$\frac{\eth T}{d\psi} = \frac{\mathfrak{D}Q}{d\psi} - \dot{\psi}\left(\kappa \frac{d\alpha}{d\psi} + \kappa' \frac{d\alpha}{d\psi} + \&c. \right)$$
$$- \dot{\phi}\left(\kappa \frac{d\beta}{d\psi} + \kappa' \frac{d\beta'}{d\psi} + \&c. \right) - \&c. + \frac{\eth Q}{d\psi} \ldots \quad (13)$$

Hence, by (1),

$$\frac{d\xi}{dt} + \frac{\mathfrak{D}Q}{d\psi} - \dot{\psi}\left(\kappa \frac{d\alpha}{d\psi} + \kappa' \frac{d\alpha'}{d\psi} + \&c. \right)$$
$$- \dot{\phi}\left(\kappa \frac{d\beta}{d\psi} + \kappa' \frac{d\beta'}{d\psi'} + \&c. \right) - \&c. + \frac{\eth Q}{d\psi} = \Psi \ldots \quad (14)$$

Now remark that, according to the notation of (12), $\xi_0, \eta_0, \ldots$ are the momentum-components of the solids due to their own motion alone without cyclic motion of the liquid; and therefore eliminate $\xi, \eta, \ldots$ by (12) from (14). Thus we find

$$\frac{d\xi_0}{dt} + \frac{\mathfrak{D}Q}{d\psi} + \alpha\frac{d\kappa}{dt} + \alpha'\frac{d\kappa'}{dt} + \&c.$$
$$+ \dot{\phi}\left\{ \kappa\left(\frac{d\alpha}{d\phi} - \frac{d\beta}{d\psi} \right) + \kappa'\left(\frac{d\alpha'}{d\phi} - \frac{d\beta'}{d\psi} \right) + \&c. \right\}$$
$$+ \dot{\theta}\left\{ \kappa\left(\frac{d\alpha}{d\theta} - \frac{d\gamma}{d\psi} \right) + \kappa'\left(\frac{d\alpha'}{d\phi} - \frac{d\gamma'}{d\psi} \right) + \&c. \right\}$$
$$+ \quad \&c. \qquad\qquad\qquad = \Psi - \frac{\eth Q}{d\psi}, \ldots \quad (15)$$

which, with the corresponding equation for ξ_0 &c., and with (11) for κ, κ', &c., are the desired equations of motion.

6. The hypothetical mode of application of K, K',... (§ 1) is impossible; and every other (such as the influence of gravity on a real liquid at different temperatures in different parts) is impossible for our ideal "liquid"—that is to say, a homogeneous incompressible perfect fluid. Hence we have K=0, K'=0, and from (11) conclude that κ, κ',... are constants. [They are sometimes called the "cyclic constants" (V. M.§§ 62–64).] The equation of motion (15) thus becomes simply

$$\frac{d\xi_0}{dt} + \frac{\mathcal{D}Q}{d\phi} + \dot{\phi}\left\{\kappa\left(\frac{d\alpha}{d\phi} - \frac{d\beta}{d\psi}\right) + \kappa'\left(\frac{d\alpha'}{d\phi} - \frac{d\beta'}{d\psi}\right) + \dots\right\}$$
$$+ \dot{\theta}\left\{\kappa\left(\frac{d\alpha}{d\theta} - \frac{d\gamma}{d\psi}\right) + \kappa'\left(\frac{d\alpha'}{d\theta} - \frac{d\beta'}{d\psi}\right) + \dots\right\}$$
$$+ \quad \text{\&c.} \qquad\qquad\qquad = \Psi - \frac{\mathcal{D}Q}{d\psi}, \dots \quad (16)$$

with corresponding equations for η_0, ζ_0, and with the following relations from (7) between ξ_0, η_0, ... and $\dot{\psi}$, $\dot{\phi}$, ...

$$\frac{dQ}{d\xi_0} = \dot{\psi}, \quad \frac{dQ}{d\eta_0} = \dot{\phi}, \quad \frac{dQ}{d\zeta_0} = \dot{\theta}, \text{ \&c.} \quad . \quad . \quad . \quad (17)$$

7. Let

$$\kappa\left(\frac{d\alpha}{d\phi} - \frac{d\beta}{d\psi}\right) + \kappa'\left(\frac{d\alpha'}{d\psi} - \frac{d\beta'}{d\psi}\right) + \text{\&c. be denoted by } \{\phi, \psi\}, \quad (18)$$

so that we have

$$\{\phi, \psi\} = -\{\psi, \phi\}. \quad . \quad . \quad . \quad (19)$$

These quantities, $\{\phi, \psi\}$, $\{\theta, \psi\}$, &c., linear functions of the cyclic constants, with coefficients depending on the configuration of the system, are to be generally regarded simply as given functions of the coordinates ψ, ϕ, θ, ...; and the equations of motion are

$$\left.\begin{array}{l}\dfrac{d\xi_0}{dt} + \dfrac{\mathcal{D}Q}{d\psi} + \{\phi, \psi\}\dot{\phi} + \{\theta, \psi\}\dot{\theta} + \text{\&c.} = \Psi - \dfrac{\mathcal{D}Q}{d\psi}, \\[2mm] \dfrac{d\eta_0}{dt} + \dfrac{Q}{d\psi} - \{\phi, \psi\}\dot{\psi} + \{\theta, \phi\}\dot{\theta} + \text{\&c.} = \Theta - \dfrac{\mathcal{D}Q}{d\theta}.\end{array}\right\} \quad . \quad (20)$$

In these (being of the Hamiltonian form) Q is regarded as a quadratic function of ξ_0, η_0, ζ_0 ... with its coefficients functions of ψ, ϕ, θ, &c.; and $\mathcal{D}$ applied to it indicates variations of these coefficients. If now we eliminate ξ_0, η_0, ζ_0, ... from Q by the

linear equations, of which (17) is an abbreviated expression, and so have Q expressed as a quadratic function of $\dot{\psi}, \dot{\phi}, \dot{\theta}, \ldots$, with its coefficients functions of ψ, ϕ, θ, &c.; and if we denote by $\dfrac{dQ}{d\phi}, \dfrac{dQ}{d\psi}$, &c. variations of Q depending on variations of these coefficients, and by $\dfrac{dQ}{d\dot{\psi}}, \dfrac{dQ}{d\dot{\phi}}$, &c. variations of Q depending on variations of $\dot{\psi}, \dot{\phi}$, &c., we have [compare Thomson and Tait, § 329 (13) and (15)]

$$\xi_0 = \frac{dQ}{d\dot{\psi}}, \quad \eta_0 = \frac{dQ}{d\dot{\psi}}, \ldots$$

and

$$\frac{\mathfrak{D}Q}{d\psi} = -\frac{dQ}{d\dot{\psi}}, \quad \frac{\mathfrak{D}Q}{d\phi} = -\frac{dQ}{d\dot{\phi}}, \ldots \qquad (21)$$

and the equations of motion become

$$\frac{d}{dt}\frac{dQ}{d\dot{\psi}} - \frac{dQ}{d\psi} + \{\phi, \psi\}\dot{\phi} + \{\theta, \psi\}\dot{\theta} + \ldots = \Psi - \frac{\mathfrak{d}\mathfrak{Q}}{d\psi},$$

$$\frac{d}{dt}\frac{dQ}{d\dot{\phi}} - \frac{dQ}{d\phi} - \{\phi, \psi\}\dot{\psi} + \{\theta, \phi\}\dot{\theta} + \ldots = \Phi - \frac{\mathfrak{d}\mathfrak{Q}}{d\phi}, \qquad (22)$$

$$\frac{d}{dt}\frac{dQ}{d\dot{\theta}} - \frac{dQ}{d\theta} - \{\theta, \psi\}\dot{\psi} - \{\theta, \phi\}\dot{\phi} + \ldots = \Theta - \frac{\mathfrak{d}\mathfrak{Q}}{d\theta}.$$

The first members here are of Lagrange's form, with the remarkable addition of the terms involving the velocities simply (in multiplication with the cyclic constants) depending on the cyclic fluid motion. The last terms of the second members contain traces of their Hamiltonian origin in the symbols $\dfrac{\mathfrak{d}}{d\psi}, \dfrac{\mathfrak{d}}{d\phi}, \ldots$

8. As a first application of these equations, let $\dot{\psi} = 0, \dot{\phi} = 0,$ $\dot{\theta} = 0, \ldots$. This makes $\xi_0 = 0, \eta_0 = 0, \ldots$, and therefore also $Q = 0$; and the equations of motion (16) (now equations of equilibrium of the solids under the influence of applied forces $\Psi, \Phi,$ &c. balancing the fluid pressure due to the polycyclic motion $\kappa, \kappa', \ldots$) become

$$\Psi = \frac{\mathfrak{d}\mathfrak{Q}}{d\psi}, \quad \Phi = \frac{\mathfrak{d}\mathfrak{Q}}{d\phi}, \text{ &c.,} \qquad (23)$$

a result which a direct application of the principle of energy renders obvious (the augmentation of the whole energy produced

by an infinitesimal displacement, $\delta\psi$, is $\dfrac{d\mathfrak{Q}}{d\psi}\,\delta\psi$; and $\Psi\delta\psi$ is the work done by the applied forces). It is proved in §§ 724–730 of a volume of collected papers on electricity and magnetism soon to be published, that $\dfrac{d\mathfrak{Q}}{d\psi}, \dfrac{d\mathfrak{Q}}{d\phi}$, &c. are the components of the forces experienced by bodies of perfect diamagnetic inductive capacity placed in the magnetic field analogous* to the supposed cyclic irrotational motion. Hence the motive influence of the cyclic motion of the liquid upon the solids in equilibrium is equal and opposite to that of magnetism in the magnetic analogue.

This is proposition II. of the paper "On the Forces experienced by Solids immersed in a Moving Liquid," which relates to the forces required to keep the movable solids at rest. The present investigation shows Prop. II. of that article to be false. Compare 'Reprint,' § 740.

9. Equations (16) for the case of a single perforated movable solid undisturbed by others, agree substantially with equations (6) and (14) of my communication† to the Royal Society of Edinburgh of February 1871. The $\xi_0, \eta_0, \ldots$ of the present article correspond to the $\dfrac{d\mathrm{T}}{du}, \dfrac{d\mathrm{T}}{dv}$, &c. of the former; the $\xi, \eta, \ldots$ mean the same in both. The equations now demonstrated constitute an extension of the theory not readily discovered or proved by that simple consideration of the principle of momentum, and moment of momentum, on which alone was founded the investigation of my former article.

10. Going back to the analytical definition of $\mathfrak{Q}$ in § 3 (5), we see that, when none of the movable solids is perforated, this configurational function is equal to the whole kinetic energy (E) which the polycyclic motion would have were there no movable solid, diminished by the energy (W) which would be given up were the liquid, which on this supposition flows through the space of the movable solid or solids, suddenly rigidified and brought to rest. Putting then

$$\mathfrak{Q}=\mathrm{E}-\mathrm{W}, \quad \cdots \quad \cdots \quad (24)$$

and remarking that E is independent of the coordinates of the movable solids, we may put $-\mathrm{W}$ in place of $\mathfrak{Q}$ in the equations

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* Proposition I. of article "On the Forces experienced by Solids immersed in a Moving Liquid," Proc. Roy. Soc. Edinb. February 1870, reprinted in Volume of Electric and Magnetic Papers, §§ 733–740.

† See Proc. Roy. Soc. Edinb. Session 1870–71, or reprint in Philosophical Magazine, November 1871.

of motion, which for this slight modification need not be written out again. W might be directly defined as the whole quantity of work required to remove the movable solids, each to an infinite distance from any other solid having a perforation with circulation through it; and, with this definition, $-W$ may be put for $\mathfrak{Q}$ in the equations of motion without exclusion of cases in which there is circulation through apertures in movable solids.

11. I conclude with a very simple case, the subject of my communication to the Royal Society of last December, in which the result was given without proof. Let there be only one moving body, and it spherical; let the perforated solid or solids be reduced to an infinitely fine immovable rigid curve or group of curves (endless, of course—that is, either finite and closed or infinite), and let there be no other fixed solid. The rigid curve or curves will be called the " core " or " cores," as their part is simply that of core for the cyclic or polycyclic motion. In this case it is convenient to take for ψ, ϕ, θ the rectangular coordinates (x, y, z) of the centre of the movable globe. Then, because the cores, being infinitely fine, offer no obstruction to the motion of the liquid, making way for the globe moving through it, we have

$$Q = \tfrac{1}{2}m(\dot{x}^2 + \dot{y}^2 + \dot{z}^2, \quad \bullet \quad \bullet \quad \bullet \quad \bullet \quad (25)$$

where m denotes the mass of the globe, together with half that of its bulk of the fluid. Hence

$$\frac{dQ}{dx} = 0, \quad \frac{dQ}{dy} = 0, \quad \frac{dQ}{dz} = 0,$$

and

$$\left. \xi_0\left(= \frac{dQ}{d\dot{x}}\right) = m\dot{x}, \quad \eta_0 = m\dot{y}, \quad \zeta_0 = m\dot{z}. \right\} \quad \bullet \quad (26)$$

A further great simplification occurs, because in the present case $\alpha d\psi + \beta d\phi + \ldots$, or, as we now have it, $\alpha dx + \beta dy + \gamma dz$ is a complete differential*. To prove this, let V be the velocity-potential at any point (a, b, c) due to the motion of the globe, irrespectively of any cyclic motion of the liquid. We have

$$V = \frac{1}{2} r^3 \left(\dot{x}\frac{d}{dx} + \dot{y}\frac{d}{dy} + \dot{z}\frac{d}{dz} \right) \frac{1}{D},$$

where r denotes the radius of the globe, and

$$D = \{(x-a)^2 + (y-b)^2 + (z-c)^2\}^{\frac{1}{2}}.$$

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* Which means that if the globe, after any motion whatever, great or small, comes again to a position in which it has been before, the integral quantity of liquid which this motion has caused to cross any fixed area is zero.

Hence, if N denote the component velocity of the liquid at (a, b, c) in any direction λ, μ, ν, we have

$$\mathrm{N} = \left(\dot{x} \frac{d}{dx} + \dot{y} \frac{d}{dy} + \dot{z} \frac{d}{dz} \right) \mathrm{F}(x, y, z, a, b, c), \quad . \quad . \quad (27)$$

where

$$\mathrm{F}(x, y, z, a, b, c) = \frac{1}{2} r^3 \left(\lambda \frac{d}{da} + \mu \frac{d}{db} + \nu \frac{d}{dc} \right) \frac{1}{\mathrm{D}}.$$

Let now (a, b, c) be any point of the barrier surface Ω (§ 2), and λ, μ, ν the direction-cosines of the normal. By (2) of § 2 we see that the part of $\dot{\chi}$ due to the motion of the globe is $\iint \mathrm{N} d\sigma$, or, by (26),

$$\left(\dot{x} \frac{d}{dx} + \dot{y} \frac{d}{dy} + \dot{z} \frac{d}{dz} \right) \iint \mathrm{F}(x, y, z, a, b, c) d\sigma. \quad . \quad (28)$$

Hence, putting

$$\iint \mathrm{F}(x, y, z, a, b, c) d\sigma = \mathrm{U},$$

we see by (8) of § 4 that

$$\alpha = -\frac{d\mathrm{U}}{dx}, \quad \beta = -\frac{d\mathrm{U}}{dy}, \quad \gamma = -\frac{d\mathrm{U}}{dz}. \quad . \quad . \quad (29)$$

Hence with the notation of § 7 (18) for $x, y, \ldots$ instead of $\psi, \phi, \ldots$

$$\{y, z\} = 0, \quad \{z, x\} = 0, \quad \{x, y\} = 0.$$

By this and (25) the equations of motion (22) with (24) become simply

$$m \frac{d^2 x}{dt^2} = \mathrm{X} + \frac{d\mathrm{W}}{dx}, \quad m \frac{d^2 y}{dt^2} = \mathrm{Y} + \frac{d\mathrm{W}}{dy}, \quad m \frac{d^2 z}{dt^2} = \mathrm{Z} + \frac{d\mathrm{W}}{dz}. \quad (30)$$

These equations express that the globe moves as a material particle of mass m, with the forces $(\mathrm{X}, \mathrm{Y}, \mathrm{Z})$ expressly applied to it, would move in a " field of force," having W for potential.

12. The value of W is, of course, easily found by aid of spherical harmonics from the velocity-potential, P, of the polycyclic motion which would exist were the globe removed, and which we must suppose known ; and in working it out (see next paragraph) it is readily seen that if, for the hypothetical undisturbed motion q denote the fluid velocity at the point really occupied by the centre of the rigid globe, we have

$$\mathrm{W} = \tfrac{1}{2} \mu q^2 + w, \quad . \quad . \quad . \quad . \quad . \quad (31)$$

where μ denotes once and a half the volume of the globe, and w denotes the kinetic energy of what we may call the internal motion of the liquid occupying for an instant in the un-

disturbed motion the space of the rigid globe in the real system. To define w, remark that the harmonic analysis proves the velocity of the centre of inertia of an irrotationally moving liquid globe to be equal to q, the velocity of the liquid at its centre*; and consider the velocity of any part of the liquid sphere, relatively to a rigid body moving with the velocity q. The kinetic energy of this relative motion is what is denoted by w. Remark also that if, by mutual forces between its parts, the liquid globe were suddenly rigidified, the velocity of the whole would be equal to q; and that $\frac{1}{2}mq^2$ is the work given up by the rigidified globe and surrounding liquid when the globe is suddenly brought to rest, being the same as the work required to start the globe with velocity q from rest in a motionless liquid.

Let $P + \psi$ be the velocity-potential at (x, y, z) in the actual motion of the liquid when the rigid globe is fixed. Let a be the radius of the globe, r distance of (x, y, z) from its centre, and $\iint d\sigma$ integration over its surface. At any point of the surface of the instantaneous liquid globe the component velocity perpendicular to the spherical surface in the undisturbed motion is $\left(\dfrac{dP}{dr}\right)_{r=a}$; and hence the impulsive pressure on the spherical surface required to change the velocity-potential of the external liquid from P to $P + \psi$, being $-\psi$, undergoes an amount of work equal to

$$\iint d\sigma \psi \cdot \frac{1}{2} \frac{dP}{dr}$$

in reducing the normal component from that value to zero. On the other hand, the internal velocity-potential is reduced from P to zero; and the work undone in this process is

$$\iint d\sigma P \cdot \frac{1}{2} \frac{dP}{dr}.$$

Hence

$$W = \frac{1}{2} \iint d\sigma (P + \psi) \frac{dP}{dr}. \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (32)$$

The condition that with velocity-potential $P + \psi$ there is no flow perpendicular to the spherical surface gives

$$\left(\frac{dP}{dr} + \frac{d\psi}{dr}\right)_{r=a} = 0. \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (33)$$

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* This follows immediately from the proposition (Thomson and Tait's 'Natural Philosophy,' § 496) that any function V, satisfying Laplace's equation $\dfrac{d^2V}{dx^2} + \dfrac{d^2V}{dy^2} + \dfrac{d^2V}{dz^2}$ throughout a spherical space, has for its mean value through this space its value at the centre; for $\dfrac{dP}{dx}$ satisfies Laplace's equation.

Now let

$$P = P_0 + P_1 \frac{r}{a} + \ldots + P \left(\frac{r}{a}\right)^i + \&c. \\ \psi = \Psi_1 \left(\frac{a}{r}\right)^2 + \ldots + \Psi_i \left(\frac{a}{r}\right)^{i+1} + \&c. \Bigg\} \quad \ldots \quad (34)$$

be the spherical harmonic developments of P and ψ relatively to the centre of the rigid globe as origin—the former necessarily convergent throughout the largest spherical space which can be described from this point as centre without enclosing any part of the core, the latter necessarily convergent throughout space external to the sphere. By (33) we have

$$\Psi_i = \frac{i}{i+1} P_i. \quad \ldots \quad \ldots \quad (35)$$

Hence (32) gives

$$W = \frac{1}{2} \iint d\sigma \left(\Sigma \frac{2i+1}{i+1} P_i \right) (\Sigma i P_i),$$

which, by

$$\iint d\sigma P_i P_{i'} = 0,$$

becomes

$$W = \frac{1}{2a} \Sigma \frac{i(2i+1)}{i+1} \iint d\sigma P_i^2. \quad \ldots \quad (36)$$

Now, remarking that a solid spherical harmonic of the first degree may be any linear function of x, y, z, put

$$P_1 \frac{r}{a} = Ax + By + Cz, \quad \ldots \quad \ldots \quad (37)$$

which gives

$$q^2 = A^2 + B^2 + C^2,$$

and

$$\frac{1}{a} \iint d\sigma P_1^2 = (A^2 + B^2 + C^2) \cdot \frac{a}{3} \cdot \iint d\sigma = q^2 \times \text{vol. of globe} = \frac{2}{3} \mu q^2.$$

Hence, by (36),

$$W = \frac{1}{2} \mu q^2 + \frac{1}{2} \iint d\sigma \left(\frac{2 \cdot 5}{3} P_2^2 + \frac{3 \cdot 7}{4} P_3^2 + \ldots \right); \quad (38)$$

and therefore, by comparison with (31),

$$w = \frac{1}{2} \iint d\sigma \left(\frac{2 \cdot 5}{3} P_2^2 + \frac{3 \cdot 7}{4} P_3^2 + \ldots \right). \quad \ldots \quad (39)$$

13. When the radius of the globe is infinitely small,

$$W = \tfrac{1}{2} \mu q^2, \quad \ldots \quad \ldots \quad \ldots \quad (40)$$

where μ denotes once and a half the volume of the globule, and q the undisturbed velocity of the fluid in its neighbourhood.

This corresponds to the formula which I gave twenty-five years ago for the force experienced by a small sphere (whether of ferromagnetic or diamagnetic non-crystalline substance) in virtue of the inductive influence which it experiences in a magnetic field*.

14. By taking an infinite straight line for the core a simple but very important example is afforded. In this case the undisturbed motion of the fluid is in circles having their centres in the core (or axis as we may now call it) and their planes perpendicular to it. As is well known, the velocity of irrotational revolution round a straight axis is inversely proportional to distance from the axis. Hence the potential function W for the force experienced by an infinitesimal solid sphere in the fluid is inversely as the square of the distance of its centre from the axis; and therefore the force is inversely as the cube of the distance, and is towards the nearest point of the axis. Hence, when the globule moves in a plane perpendicular to the axis, it describes one or other of the forms of Cotesian spirals†. If it be projected obliquely to the axis, the component velocity parallel to the axis will remain constant, and the other component will be unaffected by that one; so that the projection of the globule on the plane perpendicular to the axis will always describe the same Cotesian spiral as would be described were there no motion parallel to the axis. If the globule be left to itself in any position, it will commence moving towards the axis as if attracted by a force varying inversely as the cube of the distance. It is remarkable that it traverses at right angles an increasing liquid current without any applied force to prevent it from being (as we might erroneously at first sight expect it to be) carried sideways with the augmented stream. A properly trained dynamical intelligence would at once perceive that the constancy of moment of momentum round the axis requires the globule to move directly towards it.

15. Suppose now the globule to be of the same density as the liquid. If (being infinitely small) it is projected in the direction and with the velocity of the liquid's motion, it will move round the axis in the same circle with the liquid; but this motion would be unstable [and the neglected term w (39) adds to the

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* "On the Forces experienced by small Spheres under Magnetic Influence, and some of the Phenomena presented by Diamagnetic Substances," Cambridge and Dublin Mathematical Journal, May 1847; and "Remarks on the Forces experienced by Inductively Magnetized Ferromagnetic or Diamagnetic Non-crystalline Substances," Phil. Mag. October 1850. Reprint of papers on Electrostatics and Magnetism, §§ 634–668. Macmillan, 1872.

† Tait and Steele's 'Dynamics of a Particle,' § 149 (15).

instability]. Compare Tait and Steele's 'Dynamics of a Particle,' § 149 (15), Species IV., case A=0 and AB finite; also limiting variety between Species I. and Species V. The globule will describe the same circle in the opposite direction if projected with the same velocity opposite to that of the fluid. If the globule be projected either in the direction of the liquid's motion or opposite to it with a velocity less than that of the liquid, it will move along the Cotesian spiral (Species I. of Tait and Steele) from apse to centre in a finite time with an infinite number of turns. If it be projected in either of those directions with a velocity greater by v than that of the liquid, it will move along the Cotesian spiral (Species V. of Tait and Steele) from apse to asymptote. Its velocity along the asymptote at an infinite distance from the axis will be

$$\sqrt{v\left(v + \frac{\kappa}{\pi a}\right)},$$

and the distance of the asymptote from the axis will be

$$a \frac{v + \frac{\kappa}{2\pi a}}{\sqrt{v\left(v + \frac{\kappa}{\pi a}\right)}},$$

where a denotes the distance of the apse from the axis; and $\frac{\kappa}{2\pi a}$ the velocity of the liquid at that distance from the axis. If the globule be projected from any point in the direction of any straight line whose shortest distance from the axis is p, it will be drawn into the vortex or escape from it, according as the component velocity in the plane perpendicular to the axis is less or greater than $\frac{\kappa}{2\pi p}$. It is to be remarked that, in every case in which the globule is drawn in to the axis (except the extreme one in which its velocity is infinitely little less than that of the fluid, and its spiral path infinitely nearly perpendicular to the radius vector), the spiral by which it approaches, although it has always an infinite number of convolutions, is of finite length, and therefore, of course, the time taken to reach the axis is finite. Considering, for simplicity, motion in a plane perpendicular to the axis, at any point infinitely distant from the axis let the globule be projected with a velocity v along a line passing at distance p on either side of the axis. Then if τ denote the velocity of the fluid at distance unity from the axis [which is equal to $\frac{\kappa}{2\pi}$], and if we put

$$n^2 = 1 - \frac{\tau^2}{v^2 p^2}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (41)$$

the polar equation of the path is

$$r = \frac{np}{\cos n\theta}. \quad . \quad . \quad . \quad . \quad . \quad (42)$$

Hence the nearest approach to the axis attained by the globule is np, and the whole change of direction which it experiences is $\pi\left(\frac{1}{n} - 1\right)$. The case of $\frac{1}{n} = 2\cdot3$ is represented in the annexed diagram, copied from Tait and Steele's book [§ 149 (15), Species V.].
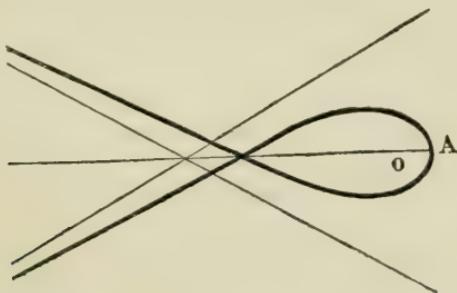
XLII. *On the Absorption of the Chemically Active Rays in the Sun's Atmosphere.* By H. C. VOGEL[*].

IT has been observed by Bouguer that the light emitted by the sun is less intense near the edges of the solar disk than in the middle, in consequence of the absorption in the atmosphere surrounding the sun. Photometric measurements showed that the intensity of the light emitted by a point situated in the centre of the solar disk is to that emitted by a point situated at a distance of three fourths of the solar radius from the centre as 48 is to 35. Observations to this effect have been made lately by Secchi[†] and Liais[‡]. Secchi has extended these observations to the heat-rays, the absorption of which he tried to determine.

It might be of interest to extend the observations to the chemically active and more refrangible rays. Although every photographic image of the sun shows a very strong decrease of light towards the border of the sun's disk, the absorbing-power of the sun's atmosphere for chemically active rays has never been

† *Le Soleil*, p. 121.
‡ " Sur l'intensité relative de la Lumière dans les divers points du disque du Soleil," *Mém. Cherbourg*, vol. xii. (1866) pp. 277–342.

determined. I have endeavoured to solve this question, and take the liberty to communicate now my observations.

Herschel[*], Claudet[†], and others have already tried to determine the intensity of light by photographic colouring; but the experiments did not succeed in consequence of the difficulty of preparing a photographic layer of the same sensitiveness throughout, and of discovering a law connecting the time of exposure, intensity of light, and the shade of tint of the photographic paper. The photochemical researches of Bunsen and Roscoe[‡] have even shown that no proportionality exists between the intensity of the light and the tint of the photographic paper after a certain time of insolation. Their observations, which were made with great care, showed *"that equal products of the intensity of the light into the times of insolation correspond within very wide limits to equal shades of tints produced on chloride-of-silver paper of uniform sensitiveness."* Calling the intensity of light I, and the time of insolation t, the equation $It = I_1 t_1$ exists for equal shades. By the discovery of this important law we are enabled to express the intensity of light in comparable measures.

The observations of Bunsen and Roscoe only relate to the chloride-of-silver paper, but do not remain valuable for photographs on collodion, since the development of the image on collodion changes the relative intensity of the shades, as is known to every photographer. I have therefore prepared photographs of the sun on chloride-of-silver paper in order to measure the intensity of the light emitted by different points of the sun's disk.

The paper was carefully prepared, stretched over a plate of glass, and exposed in the camera fixed to the telescope. Two images of the sun were prepared on the 8th of March, 1872; one was exposed during 30 seconds, the other during 40 seconds. The diameter of the images was 108 millims.; the weakening of the light towards the edge of the disk was clearly seen in both. I then determined four points of equal intensity by a scale prepared photographically[§]. We assume that the intensity i of the light used for the preparation of the scale was constant, and that the time t of exposure was the same for all points of the sun's image. If I_0 is the intensity of the light in the middle of the sun's disk, I_1 that at another point, we have

$$I_0 t = it_1,$$
$$I_1 t = it_2,$$

[*] Phil. Trans. 1840, p. 46.   [†] Phil. Mag. vol. xxxiii. p. 339.

[‡] Pogg. *Ann.* vol. cxvii. p. 329.

[§] I think it superfluous to give a detailed description of the manner in which the scale was prepared and the comparison with the sun's image made, as the method was in general the same as that used by Bunsen and Roscoe, and described by them accurately in their 'Photochemical Researches.'

and therefore

$$I_0 : I_1 = t_1 : t_2.$$

I have collected the observations, which seem to me pretty consistent, in the following Table. The numbers given by the readings of the scale have been reduced so as to make the intensity in the middle of the disk equal to 100.

Distance from centre.	Intensities.	
Radius = 54.	First image.	Second image.
0	100·0*	100·0
4	100·0	99·0
8	......	97·9
9	99·0	
11	98·9	
12	......	100·0
13	97·9	
15	97·9	
16	......	95·8
17	95·6	
19	95·6	
20	......	95·0
21	93·4	
23	93·4	
24	......	90·1
25	93·3	
27	90·4	
28	.....	85·4
29	85·9	
31	83·7	
32	......	84·5
33	78·5	
35	77·2	
36	......	78·1
37	75·0	
39	68·8	
40	.....	67·7
41	64·6	
43	60·3	
44	......	56·4
45	53·1	
46	......	48·4
47	45·8	......
48	......	41·5
49	37·3	
50	......	25·3
51	29·8	
52	......	18·0
53	15·9	10·1
53·5	10·7	

The observations were repeated on the 13th of March. Two

* In the observations the relative intensities were only expressed by whole numbers; the decimals in the Table arise from the reduction.

sheets of paper were again exposed during 30 and 40 seconds. The numbers given in the following Table were calculated by taking the mean of four observations, which were taken by determining the intensity on four points at equal distances from the sun, and situated on two diameters at right angles to each other.

Distance from centre. Radius = 54.	Intensities.
0	100·0
4	100·0
14	98·9
24	94·3
34	78·5
40	70·0
45	53·9
47	40·0
49	32·6
51	28·6
53	18·7

In the first days of the month of May numerous observations were made on photographs taken in the focus of the large equatorial instrument, which have only a diameter of 45·4 millims. In order to be able to compare these observations with those just given, I have assumed their radius to be 54, and reduced the observations by multiplying them with $\frac{54}{22\cdot7}$. The numbers given are the means of eight observations on six photographs.

Distance from centre. Radius = 54.	Intensities.
0·0	100·0
11·9	98·3
23·8	93·0
35·7	78·0
47·6	49·9
51·6	22·0
52·8	14·5

On the 5th of May a magnified photograph was taken, and the relative intensity was measured on points lying symmetrically from the centre on a diameter. The diameter of the photograph was 102·9. The tints were compared with two scales prepared at different times.

Distance from centre.	Intensities.	
Radius = 54.	Scale 1.	Scale 2.
0·0	100·0	100·0
10·5	98·4	99·0
21·0	90·0	94·1
31·5	84·3	86·3
41·9	65·9	68·3
47·2	49·0	48·0
52·5	28·1	30·7
53·5	18·8	19·5

I have tried to determine the relative intensities of the larger sun-spots, their penumbræ, and the adjoining surface of the sun. The mean of several observations showed the ratio of the intensity of the spot to the adjoining parts of the sun's surface to be 0·067; that of the penumbra was 0·630*.

If we take the distance from the centre as abscissa, the intensity as ordinate, we obtain a curve showing the decrease of the intensity from the centre of the sun's disk. The ordinates I of the curve which agrees best with the observations have been determined as follows for the distances E from the centre :—

E = sin θ.	θ.	I.
Radius = 12.		
0	0° 0·0	100·0
1	4 46·8	100·0
2	9 35·7	99·4
3	14 28·6	98·2
4	19 28·3	96·4
5	24 37·5	93·7
6	30 0·0	89·8
7	35 41·2	84·5
8	41 48·6	77·0
9	48 35·4	66·0
10	56 26·6	51·0
11	66 26·6	33·0
12	90 0·0	13·5

If we compare the above observations with those made by Liais, Secchi, and others for the less-refrangible rays (*i. e.* the optically active heating rays), we see that the absorption for chemically active rays is considerably larger.

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* The observations must only be considered preliminary, and will be repeated as soon as large sun-spots appear. Liais (*l. c.* p. 327) determines the ratio of the intensity of light of a spot to the adjoining parts of the sun's disk to be 0·091 for rays of mean refrangibility, and that of the penumbra 0·5.

If these researches were executed for homogeneous rays, we might obtain interesting information as to the constitution of the sun's atmosphere.

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The author of the above paper seems to have been unaware of the observations taken by Professor Roscoe in 1863 and communicated to the Royal Society (see Phil. Mag. vol. xxvii. p.384). Professor Roscoe has proved that the intensity of the chemically active rays at the centre of the sun is greater than at the edge of the disk. The absorption of the sun's atmosphere, according to Professor Roscoe's experiments, is not so large as that resulting from Vogel's observations. Calling the intensity at the centre of the solar disk 100, the mean of Roscoe's observations gives about 28 for the intensity at the edge, while Vogel gives 13. The discrepancy may be partly accounted for by the rapid decrease in the intensity near the edge; but it is likely that this is not the full explanation of it. Vogel does not state whether he used a refracting or a reflecting telescope. Suppose he used a reflecting one, the mean wave-length of the rays the intensity of which he measured would be smaller than that of the rays measured by Roscoe, who used a refracting telescope; and if this should be so, it will add another proof of the fact that the smaller the wave-length the greater is its absorption.—ARTHUR SCHUSTER, Ph.D.

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XLIII. *On the Effects of Magnetization in changing the Dimensions of Iron, Steel, and Bismuth bars, and in increasing the Interior Capacity of Hollow Iron Cylinders.*—Part I. *By* ALFRED M. MAYER, *Ph.D., Professor of Physics in the Stevens Institute of Technology, Hoboken, New Jersey, U.S.A.*[*].

I PURPOSE giving, in a series of papers, the results of a prolonged and careful research on the above subject.[:]

Introduction.—In 1842 Joule discovered that when a current of electricity was passed through a helix which enclosed a bar of iron, the latter, on its magnetization, suddenly elongated a minute fraction of its length.

To present clearly Dr. Joule's experiments, we will give these abstracts from the excellent paper which he published in the Philosophical Magazine in 1847:—

"In order to ascertain how far my opinion as to the invariability of the *bulk* of a bar of iron under magnetic influence was well founded, I devised the following apparatus. Ten copper

wires, each 110 yards long and $\frac{1}{20}$ of an inch in diameter, were bound together by tape so as to form a good and at the same time very flexible conductor. The bundle of wires thus formed was coiled upon a glass tube 40 inches long and $1\frac{1}{2}$ inch in diameter. One end of the tube was hermetically sealed; and the other end was furnished with a glass stopper, which was itself perforated so as to admit of the insertion of a capillary tube. In making the experiments, a bar of annealed iron, 1 yard long and $\frac{1}{2}$ an inch square, was placed in the tube, which was then filled up with water. The stopper was then adjusted, and the capillary tube inserted so as to force the water to a convenient height within it.

"The bulk of the iron was about 4,500,000 times the capacity of each division of the graduated tube; consequently a very minute expansion of the former would have produced a very perceptible motion of the water in the capillary tube; but, on connecting the coil with a Daniell's battery of five or six cells (a voltaic apparatus quite adequate to saturate the iron), no perceptible effect whatever was produced either in making or breaking contact with the battery, whether the water was stationary in the stem, or gradually rising or falling from a change of temperature. Now, had the usual increase of length been unaccompanied by a corresponding diminution of the diameter of the bar, the water would have been forced through twenty divisions of the capillary tube every time that contact was made with the battery.

"Having thus ascertained that the bulk of the bar was invariable, I proceeded to repeat my first experiments with a more delicate apparatus, in order, by a more careful investigation of the laws of the increment of length, to ascend to the probable cause of the phenomenon.

"A coiled glass tube, similar to that already described, was fixed vertically in a wooden frame. Its length was such that when a bar 1 yard long was introduced so as to rest on the sealed end, each extremity of the bar was a full inch within the corresponding extremity of the coil. The apparatus for observing the increment of length consisted of two levers of the first order, and a powerful microscope situated at the extremity of the second lever. These levers were furnished with brass knife-edges resting upon glass. The connexion between the free extremity of the bar of iron and the first lever, and that between the two levers, was established by means of exceedingly fine platinum wires.

"The first lever multiplied the motion of the extremity of the bar 7·8 times; the second multiplied the motion of the first 8 times; and the microscope was furnished with a micrometer di-

vided into parts each corresponding to $\frac{1}{2220}$ of an inch. Consequently each division of the micrometer passed over by the index indicated an increment of the length of the bar amounting to $\frac{1}{138528}$ of an inch.

"The quantities of electricity passing through the coil were measured by an accurate galvanometer of tangents, consisting of a circle of thick copper wire 1 foot in diameter, and a needle $\frac{1}{2}$ an inch long furnished with a suitable index.

"The quantities of magnetic polarity communicated to the iron bar were measured by a finely suspended magnet 18 inches long, placed at the distance of 1 foot from the centre of the coil. This magnetic bar was furnished with scales precisely in the manner of an ordinary balance; and the weight required to bring it to a horizontal position indicated the intensity of the magnetism of the iron bar under examination.

"After a few preliminary trials, a great advantage was found to result from filling the tube with water. The effect of the water was, as De la Rive had already remarked, to prevent the sound. It also checked the oscillations of the index, and had the important effect of preventing any considerable irregularities in the temperature of the bar.

"The first experiment which I shall record was made with a bar consisting of two pieces of well-annealed rectangular iron wire, each 1 yard long, $\frac{1}{4}$ of an inch broad, and about $\frac{1}{8}$ of an inch thick. The pieces were fastened together so as to form a bar of nearly $\frac{1}{4}$ of an inch square. The coil was placed in connexion with a single constant cell, the resistance being further increased by the addition of a few feet of fine wire. The instant that the circuit was closed, the index passed over one division of the micrometer. The needle of the galvanometer was then observed to stand at $7° 20'$, while the magnetic balance required 0.52 of a grain to bring it to an equilibrium. It had been found by proper experiments that a current of $7° 20'$ passing through the coil was itself capable of exerting a force of 0.03 of a grain upon the balance; consequently the magnetic intensity of the bar was represented by 0.49 of a grain. On breaking the circuit the index was observed to retire 0.3 of a division, leaving a permanent elongation of 0.7, and a permanent polarity of 0.42 of a grain. More powerful currents were now passed through the coil and the observations repeated as before, with the results tabulated below:—

Experiment I.

Deflection of galvanometer.	Tangent of deflection.	Elongation or shortening of bar.	Total elongation.	Magnetic intensity of bar.	Square of magnetic intensity divided by total elongation.
− 7 20	128	1·0 E.	1·0	−0·49	240
0	0	0·3 S.	0·7	−0·12	252
− 9 30	167	2·9 E.	3·6	−0·93	240
0	0	1·2 S.	2·4	−0·74	228
−14 48	264	5·9 E.	8·3	−1·42	243
0	0	3·8 S.	4·5	−1·00	222
−23 10	428	10·3 E.	14·8	−1·87	236
0	0	7·6 S.	7·2	−1·26	220
−47 25	1088	16·1 E.	23·3	−2·22	211
0	0	13·9 S.	9·4	−1·35	194
−58 50	1653	14·8 E.	24·2	−2·21	202
0	0	12·3 S.	10·9	−1·35	168"

Dr. Joule now reversed the current in the helix and found that a current which deflected the needle 6° 15′ *shortened* the bar 3·4 div., and that after the current was broken its magnetic intensity was found reduced from −1·3 (the permanent intensity previously given by 47° 25′, see preceding Table) to −·17. He then passed a current of 9° 55′; and this he found was sufficient, not only to remove the former minus polarity of the bar, but also to give it a permanent polarity of + ·25, and yet to leave the bar with 6·6 of the elongation belonging to its previous minus polarity.

Taking Joule's observations while the current was passing around the bar, we have for the current of 6° 15′ a magnetic intensity of −0·12, and for the current of 9° 15′ a *plus* magnetic polarity of 0·57. We call attention to these results because subsequent experimenters* seem to be unaware of these observations of Dr. Joule, who here first shows that a feeble current will demagnetize and even reverse the polarity of a bar which has previously required a far more powerful current to give it its permanent magnetic charge. In the experiment given above, the ratio of the current-intensities of permanent magnetization and of demagnetization is 1088 to 175.

Dr. Joule now successively replaced the above bar by two others, and obtained with them similar results. He then deduces the following important law :—" From the last column of each of the preceding Tables we may, I think, safely infer that

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* Wiedemann, Pogg. *Ann.* vol. c. p. 235 ; also R. W. Wilson, " Demagnetization of Electromagnets," American Journal of Science, vol. iii. 3rd Series, p. 346.

the elongation is in the duplicate ratio of the magnetic intensity of the bar, both when the magnetism is maintained by the influence of the coil, and in the case of the permanent magnetism after the current has been cut off.   The discrepancies observable will, I think, be satisfactorily accounted for when we consider the nature of the magnetic actions taking place.   When a bar experiences the inductive influence of a coil traversed by an electrical current, the particles near its axis do not receive as much polarity as those near its surface, because the former have to withstand the opposing inductive influence of a greater number of magnetic particles than the latter.   This phenomenon will be diminished in the extent of its manifestation with an increase of the electrical force, and will finally disappear when the current is sufficiently powerful to saturate the iron.   Again, when the iron, after having been magnetized by the coil, is abandoned to its own retentive powers by cutting off the electrical current, the magnetism of the interior particles will suffer a greater amount of deterioration than that of the exterior particles.   The polarity of the former may indeed be sometimes actually reversed, as Dr. Scoresby found it to be in some extensive combinations of steel bars.   Now, whenever such influences as the above occur, so as to make the different parts of the bar magnetic to a various extent, the elongation will necesarily bear a greater proportion to the square of the magnetic intensity measured by the balance than would otherwise be the case.

" For similar causes the interior of the bar will in general receive the neutralization and reversion of its polarity before the exterior ; and hence we see in the Tables that there is a considerable elongation of the bar after the reversion of the current, even when the effect upon the balance has become imperceptible, owing to the opposite effects of the interior and exterior magnetic particles."

Joule now experimented on a bar of unannealed iron, and on three bars of soft steel.   As these bars had considerable degrees of retentive power, the anomalies occasioned by the above-described actions did not exist to any considerable extent, and they gave a confirmation of the law that the elongation is proportional, in a given bar, to the square of the magnetic intensity.

The next bar he experimented with was of moderately hardened steel.   This bar was slightly increased in length every time that contact with the battery was broken, although a considerable diminution of the magnetism of the bar took place at the same time.   He says :—" I am disposed to attribute this effect to the state of tension in the hardened steel, for I find that soft iron wire presents a similar anomaly when stretched tightly."

In a subsequent communication, contained in the same vo-

lume of the Philosophical Magazine, Dr. Joule gives accounts of numerous experiments made upon wires and bars of soft iron, cast iron, soft and hardened steel, subjected to various pressures and tensions while they were magnetized. As an example of the effect of *tension* on the phenomena, he states that in the case of a bar 1 foot long and $\frac{1}{4}$ of an inch in diameter, a tensile force of about 600 pounds caused all the phenomena of changes of length to disappear, even with a current which produced a deflection of 58° in the needle of the tangent-galvanometer; but when a current of 61° was passed around this bar, subjected to a tension of 1040 pounds, it *shortened* 2·8 divisions. With a tension of 1680 and the same current the bar shortened 4·5 divisions. Joule, from his experiments, deduces this law; viz. *In the case of tension the shortening effect is proportional to the current traversing the coil multiplied by the magnetic intensity of the bar.* He further states that "it is extremely probable that the shortening effects are proportional, *cæteris paribus*, to the square root of the force of tension."

In the case of bars of cast iron he finds that their elongation is equal, if not superior, to those of soft iron when magnetized to the same degree; and an increase of tension in them does not produce half the retraction which is caused in soft iron bars in similar circumstances.

Bars of soft steel acted like the bars of iron; but the superior retentive powers of the former enabled him to trace better the elongating effects of the permanent magnetism, which diminished with the increase of tension and at last disappeared altogether; but with bars of perfectly hardened steel no sensible change in their lengths was produced by charges of *permanent* magnetism, and the *temporary* shortening effect of the coil was proportional to the magnetism multiplied by the current traversing the coil. The shortening effect did not in these cases sensibly increase with the increase of tension.

On subjecting bars of wrought and cast iron and soft steel to pressure, Joule found that it had no sensible effect upon the extent of their elongation. A hard steel cylinder a foot long, when submitted to the same experiments, with a pressure of 80 pounds, "suffered a diminution of length equal to 0·1 of a division of the micrometer, with a current capable of giving a magnetic polarity of 1·7."

At the termination of his paper Dr. Joule gives the following "*postscript.*" "I have already, in the former part of this paper, described an experiment which indicated that no alteration in the *bulk* of a bar of soft iron could be produced on magnetizing it. I thought, however, that it would be interesting to confirm the fact by an observation of the alteration of the dimensions of

the iron at right angles to the direction of its polarity. For this purpose I took a piece of drawn iron gas-piping 1 yard long, $\frac{3}{16}$ of an inch in bore, and $\frac{3}{16}$ of an inch in thickness. A piece of thick covered copper wire was inserted into this tube and bent over the outside of it. The lower extremity of the iron tube being fixed, and the upper end being attached to the micrometrical apparatus, each division of which corresponded to $\frac{1}{138528}$ of an inch, I obtained .... results which show that the length of the tube was diminished in order to make up for the increase of its diameter, which in this instance was in the direction of the polarity. The quantity of the shortening effect, viz. 3·4, is, however, only one third of that due to the maximum elongation of soft iron bars as observed in the first section. This is probably owing to the grain of the iron being in cross directions with respect to the polarity in the two cases, and partly perhaps to the iron tube not being fully saturated with magnetism. The experiment is worth repeating, especially as it affords a means of studying the magnetic condition of closed circuits."

Remarking on the cause of the phenomena of elongation, Dr. Joule says :—" The law of *elongation* naturally suggests the joint operation of the attractive and repulsive forces of the constituent particles of the magnet as the cause of the phenomena. On the other hand, the fact that the *shortening effect* is proportional to the magnetic intensity of the bar multiplied by the current traversing the coil seems to indicate that in this case the effect is produced by the attraction of the magnetic particles by the coil. But then it will be asked why so remarkable an augmentation of the effect is produced by the increase of tension in the case of the soft iron bars. When we are able to answer this question in a satisfactory manner, we shall probably have a much more complete acquaintance with the real nature of magnetism than we at present possess."

This full account of Dr. Joule's remarkable research is here presented in order to give an exposition of our present knowledge of this subject, and clearly to set forth the relations which my own attempts bear to his labours. Here Joule, the discoverer of these phenomena, has given us almost all the knowledge we have up to this time possessed in reference to their characteristics and their laws. That a subject so fascinating should not have been eagerly followed up appears strange, especially so when it seems highly probable that the faithful study of these actions may one day give us an insight into the dynamic nature of electro-magnetization, and thus lead the investigator into a fruitful field of research.

No one can duly appreciate this work of Joule's until he attempts the confirmation of his results ; then the difficulties of

the research and the skill and acumen of this eminent physicist will be properly estimated.

Although the cognate discovery by our countryman Page, in 1837, that iron bars produce sound on their magnetization, has been carefully studied by Delezenne, De la Rive, Beatson, Marrian, and Wertheim, yet in the annals of science I have found only two experimental investigations, in addition to the one by Joule, on the phenomena of the elongation produced in iron rods on their magnetization. The first is by Wertheim, in the *Ann. de Chim. et de Phys.* 3 sér. vol. xxiii.; the second by Tyndall, contained in a paper entitled "On some Mechanical Effects of Magnetization," published in his 'Researches on Diamagnetism and Magnecrystallic Action,' London, 1870.

In Wertheim's memoir "On the Sounds produced in Magnetized Iron," all we find on the subject of the elongation of magnetized iron rods is the following :—"Here are the results of these experiments : the helix being placed so that its axis coincides with that of the bar, we do not observe any lateral movement, but only a very small elongation; this elongation rarely surpasses ·002 millim. [in rods about 970 millims. long], and although visible is bearly measurable; it is most pronounced when the helix [whose length was a litttle over $\frac{1}{5}$ of that of the rod] encloses the extremity of the bar; it diminishes as the helix approaches the point [the centre] where the rod is clamped; and it is probable that when it is quite close to this point the elongation changes into a retraction; but I have never been able to observe the motion in this direction with any certainty. . . . . I have already remarked that it was not possible for me to measure this longitudinal traction; happily Mr. Joule has supplied that omission."

Dr. Tyndall opens his paper thus :—"Wishing, in 1855, to make the comparison of magnetic and diamagnetic phenomena as thorough as possible, I sought to determine whether the act of magnetization produces any change of dimensions in the case of bismuth, as it is known to do in the case of iron. The action, if any, was sure to be infinitesimal; and I therefore cast about for a means of magnifying it. . . . . I consulted Mr. Becker; and thanks to his great intelligence and refined skill, I became the possessor of the apparatus now to be described. . . . . The same apparatus has been employed in the examination of bismuth bars; and though considerable power has been applied I have hitherto failed to produce any sensible effect. It was at least conceivable that complementary effects might be here exhibited, and a new antithesis thus established between magnetism and diamagnetism."

The apparatus used by Dr. Tyndall consisted of two vertical

brass rods firmly cemented into a block of stone. Between these rods, securely fixed in the stone, were placed the rods of iron whose elongation he desired to measure. On the vertical rods slid a transverse bar of brass carrying "a vertical rod of brass which moves freely and accurately in a long brass collar. The lower end of the brass rod rests upon the upper flat surface of the iron bar. To the top of the brass rod is attached a point of steel; and this point passes against a plate of agate, near a pivot which forms the fulcrum of a lever. The distant end of the lever is connected by a very fine wire, with an axis on which is fixed a small circular mirror. If the steel point be pushed up against the agate plate, the end of the lever is raised; the axis is thereby caused to turn, and the mirror rotates." The angular deflections of the mirror he determined by the method of Poggendorff—that is, by viewing in a telescope the divisions of a fixed scale reflected from the mirror.

Dr. Tyndall gives the following account of his experience with this apparatus :—" Biot found it impossible to work at his experiments on sound during the day in Paris; he was obliged to wait for the stillness of night. I found it almost equally difficult to make accurate experiments, requiring the telescope and scale, with the instrument just described, in London. Take a single experiment in illustration. The mirror was fixed so as to cause the cross hair of the telescope to cut the number 727 on the scale : a cab passed while I was observing; the mirror quivered, obliterating the distinctness of the figure, and the scale slid apparently through the field of view and became stationary at 694. I went upstairs for a book; a cab passed, and on my return I found the cross hair at 686. A heavy waggon then passed, and shook the scale down to 420. Several carriages passed subsequently; the figure on the scale was afterwards 350. In fact so sensitive is the instrument, that long before the sound of a cab is heard its approach is heralded by the quivering of the figures on the scale.

" Various alterations which were suggested by the experiments were carried out by Mr. Becker, and the longer I worked with it the more mastery I obtained over it; but I did not work with it sufficiently long to perfect its arrangement. Some of the results, however, may be stated here :—

	Figure of scale.
Bar unmagnetized . . .	577
Bar magnetized	470
Bar unmagnetized . . .	517

" Here the magnetization of the bar produced an elongation expressed by 107 divisions of the scale, while the interruption

of the circuit produced only a shrinking of 47 divisions. There was a tendency on the part of the bar or of the mirror to persist in the condition superinduced by the magnetism. The passing of a cab in this instance caused the scale to move from 517 to 534; that is, it made the shrinking 64 instead of 47. Tapping the bar produced the same effect.

"The bar employed here was a wrought-iron square core, 1·2 inch a side and 2 feet long.

"The following Tables will sufficiently illustrate the performance of the instrument in its present condition. In each case are given the figures observed before closing, after closing, and after interrupting the circuit. Attached to each Table also are the lengthening produced by magnetizing and the shortening consequent on the interruption of the circuit :—

Circuit.	Scale 10 cells.		Circuit.	Scale. 20 cells.	
Open	647		Open	653	
Closed......	516	131 elongation.	Closed ...	465	188 elongation.
Broken ...	581	65 return.	Broken ...	579	114 return.
Open	637		Open	638	
Closed ...	509	128 elongation.	Closed ...	452	186 elongation.
Broken ...	579	70 return.	Broken ...	568	116 return.
Open	632		Open	632	
Closed ...	491	141 elongation.	Closed ...	472	160 elongation.
Broken ...	568	77 return.	Broken ...	561	89 return.

"These constitute but a small fraction of the numbers of experiments actually made. There are very decided indications that the amount of elongation depends on the molecular condition of the bar. For example, a bar taken from a mass used in the manufacture of a great gun at the Mersey Iron works, suffered changes on magnetization and demagnetization considerably less than those recorded here. I hope to return to the subject."

XLIV. *On the Intensity of Light &c.*
By Henry Hudson, *M.D., M.R.I.A.*

To the Editors of the Philosophical Magazine and Journal.

Glenville, Fermoy,
March 4, 1873.

Gentlemen,

I THINK Mr. Bosanquet could not have looked into the Astronomer Royal's excellent little work 'On the Undulatory Theory of Light' when he wrote (p. 217) that "The explanation given by Airy of the doubling of intensity . . . . can

only be regarded as an illustration." Sir George writes (p. 20), "We shall assume the intensity of the light to be represented by c^2," and (in a note) adds, "We *must* take some even power of c to represent the intensity, since the undulation where the vibration is expressed by $-c \sin \left\{ \dfrac{2\pi}{\lambda} (vt - x) + \text{C} \right\}$ differs in no respect from that whose vibration is expressed by

$$+ c \sin \left(\frac{2\pi}{\lambda} (vt - x) + \text{C} \right),$$

except that it is half the length of a wave before or behind it."

It would appear, therefore, that the Astronomer Royal was influenced to adopt the "square" of the amplitude as the measure of intensity chiefly because the *even power*s of positive and negative quantities are (*algebraically*) identical.

I would suggest, however, that the true *physical* interpretation of the signs ($+$ and $-$) prefixed respectively to two perfectly similar vibrations is that "the coefficient (c) must be measured *in opposite directions* from the point of rest of the disturbed particle," which in fact constitutes the difference, by half a wavelength, of these two similar vibrations; and it is evident (algebraically as well as physically) that the combination of any two such vibrations must produce *zero* (i. e. *darkness* in the case of light), or in optical language "*interference*."

I would now submit my view of Sir George Airy's argument (in his note, p. 20) to the consideration of mathematicians. First, let us assume (with Sir George, p. 7) that c is the "maximum vibration of the disturbed particle;" in this case (the wave-length and amplitude being identical) it appears to me that the only effect of introducing C and D into two perfectly similar vibrations is (see 'Undulatory Theory,' p. 6) to "alter the origin of the linear measure from which x is reckoned," and that no conclusion as to the "influence of amplitude on intensity" can be deduced from such a change. Secondly (c and λ being still alike in both the new forms), if we consider c to represent merely the "actual distance of the disturbed particle from its place of rest," the introduction of C and D into the vibration-formula *may* also represent a "change of phase of the wave." But (inasmuch as c no longer represents the *maximum vibration*) it will not be possible to deduce any "influence of amplitude on intensity" from the formulæ even in this case. Musical men are aware that a "pizzicato note" from a stringed instrument, even with very moderate amplitude of vibration, can be heard at a considerable distance. *Suppose* now that such a sound wave (with 1 inch amplitude) becomes insensible at 200 feet : if the amplitude be reduced to half an inch, the distance

at which the sound should no longer be audible ought to be 100 feet only if the *square* of the amplitude be the measure of its intensity, or at 141 feet if the *amplitude simply* be the correct indicator of intensity; or if the amplitude of the second wave were one third of an inch only, then the distances (according to the two hypotheses) would be about 67 feet and 115 feet respectively.

<div style="text-align:right">HENRY HUDSON.</div>

P.S.—In such an experiment we have the advantage of dealing with a single wave.

XLV. *On the Definition of Intensity in the Theories of Light and Sound.* By ROBERT MOON, *M.A., Honorary Fellow of Queen's College, Cambridge*.

IN a note upon the subject of this paper, contained in the March Number of the Philosophical Magazine, Mr. Bosanquet expresses himself as follows :—

"Mr. Moon has not offered any answer to the remark made at the end of my paper of last November, although, if he understood it, it is conclusive in the case of light."

There are some truths so obvious, some arguments which appear so decisive, that one is apt to suppose that the mere statement of them will suffice to carry conviction to the mind even of an opponent. The argument I offered to Mr. Bosanquet appeared to me precisely of that character. As he, however, regards it in a different light, I am ready to meet him upon his own ground.

I have no intention to contest the substantial approximate truth of an experimental law so long established, so *à priori* all but certain, as Malus's rule of cosines; but I demur *in toto* to Mr. Bosanquet's conclusion that the adoption of the simple power of the amplitude as the measure of intensity in plane-polarized rays involves the assumption that $a (\sin \alpha + \cos \alpha)$ measures the intensity of the overlapping beams in the experiment which he discusses. So far is this from being the fact, that the latter assumption contradicts the former, as can readily be shown. For, suppose that at a particular point where the beams overlap, the oppositely polarized rays happen to be in the same phase, as they may be; they will then give rise to a single plane-polarized ray, whose intensity would be a according to the measure which I have proposed, and not $a (\sin \alpha + \cos \alpha)$, as the measure which Mr. Bosanquet thus gratuitously seeks to fix upon me would indicate.

The inconsistency of this proposed extension of my definition

may be equally seen in the general case, where the oppositely po-
larized rays are in different phases.   For, representing the rays
by

$$\left.\begin{array}{l} a\cos\alpha \,.\, \sin X, \\ a\sin\alpha \,.\, \sin (X+D), \end{array}\right\} \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (1)$$

the first may be replaced by two oppositely polarized rays repre-
sented respectively by

$$a\cos^2\alpha \,.\, \sin X, \quad a\cos\alpha\sin\alpha \,.\, \sin X ; \quad \cdot \quad \cdot \quad \cdot \quad (2)$$

and the second by two similar rays represented by

$$a\sin^2\alpha \,.\, \sin (X+D), \quad -a\cos\alpha\sin\alpha \,.\, \sin (X+D) . \,. \quad (3)$$

Hence, combining the expressions for rays polarized in the same
plane, we shall have in place of (1) *two* waves polarized in oppo-
site planes, respectively represented by

$$a \,.\, \{ (\cos^2\alpha + \sin^2\alpha \cos D) \,.\, \sin X + \sin^2\alpha \,.\, \sin D \,.\, \cos X \},$$

$$a \,.\, \cos\alpha\sin\alpha \,.\, \{ (1 - \cos D) \sin X - \sin D \,.\, \cos X \} ;$$

which may be written

$$a\sqrt{\overline{\cos^2\alpha + \sin^2\alpha \cos D)^2} + \sin^4\alpha \,.\, \sin^2 D} \,.\, \sin (X+D_1),$$

$$a \,.\, \sin\alpha\cos\alpha \,.\, \sqrt{(1 - \cos D)^2 + \sin^2 D} \,.\, \sin (X+D_2) ;$$

or

$$a\sqrt{\cos^4\alpha + \sin^4\alpha + 2\sin^2\alpha \cos^2\alpha \cos D} \,.\, \sin (X+D_1),$$

$$a\sin\alpha\cos\alpha\sqrt{2(1 - \cos D)} \,.\, \sin (X+D_2).$$

Now we have just as much right to take the sum of the am-
plitudes of these two waves for the intensity at any point of the
overlapping beams, as we have to take the sum of the amplitudes
of the waves represented by (1) for the like purpose. A com-
parison of the results thus derivable, however, will show that
they are incompatible, and consequently that Mr. Bosanquet's
proposed extension of my definition of intensity in the case
of oppositely polarized rays cannot be entertained.

Undoubtedly, however, I may be expected to state how I pro-
pose to estimate the collective effect of the oppositely polarized
rays in the circumstances referred to; and this I shall have no
difficulty in doing.

If this collective effect is capable of being expressed by a
function of the intensities of the two waves when acting sepa-
rately, whatever be the phases of the latter, we shall have

$$\text{intensity} = F \{ a\cos\alpha), \; (a\sin\alpha) \} ;$$

and if we can discover the form of F corresponding to any par-

ticular state of phase, we shall know the form of F for all varieties of phase.

Now, when the two rays are in the same phase, we have

$$a = \text{intensity} = \mathrm{F}\{a\cos\alpha),\ (a\sin\alpha)\}\ ;$$

therefore

$$\mathrm{F}\{(a\cos\alpha),\ (a\sin\alpha)\} = \sqrt{(a\cos\alpha)^2 + (a\sin\alpha)^2}.$$

Hence, in the case under consideration, the intensity will not be measured by the sum of the amplitudes, but by *the square root of the sum of the squares of the amplitudes of the component rays.*

It thus appears that the argument which Mr. Bosanquet puts forward as decisive against the simple power of the amplitude being taken for the measure of intensity in plane-polarized waves has, in fact, no bearing upon the subject.

The measures of intensity which I have proposed as applicable to plane and elliptically polarized light coincide in a remarkable manner.

For, when the component rays are represented by (1), the resulting ray will be elliptically polarized—the magnitude and position of the axes of the ellipse depending on a, which measures the intensity of the incident light, D which represents the difference of phase of the component rays, and α the inclination of the principal plane of the crystal to the plane of polarization of the beam originally incident upon it.

The absolute magnitude of either axis will always be proportional to a, while the position of the axes and their ratio to each other depend on D and α. Hence, so long as its *form* is unaltered, the circumference of the ellipse (*i. e.* the length of path described by a particle in a single undulation) will vary as a; and the intensity of the overlapping rays for the same form of vibration will also vary as a.

It thus appears that when from the consideration of plane-polarized we turn to that of elliptically polarized light, length of path of the particles is not sufficient to determine the intensity; the form of vibration must also be taken into account; but for a fixed form of vibration the intensity varies directly as the length of path of the particles.

The single argument I adduced against Mr. Bosanquet's view of the relation of the amplitude to the intensity was that, according to the latter, two equal vibrations in the same phase will give four times as much illumination as either separately. This argument, which appears to me irrefragable, Mr. Bosanquet regards as so utterly trifling that it is only from the consideration that it is " sometimes felt as a difficulty by learners " that he is induced to "just touch upon it."

Admitting, after some hesitation, " that two vibrations may be superposed with coincident phase," he proceeds as follows :—

" In these cases we must not treat each vibration as a cause in itself, invariable under all conditions. If we regard the two vibrations as unaltered by the superposition, we shall in general be wrong."

This line of defence cuts away the ground upon which rest the undulatory theories of light and sound. Those theories alike assume the application of the principle of the superposition of small motions in all cases with which they undertake to deal. The theory of interference assumes that the motion in a wave may be represented by the ordinate of a particle—and that where two waves are superposed in which the vibration is in the same plane, their joint effect is represented by the algebraical sum of the expressions for the ordinates corresponding to each wave taken separately.

This implies that the superposition of two equal waves in the same phase will produce, as regards any particle at a given time, twice the force acting upon it, twice the velocity impressed upon it, twice the space described by it, which would occur if either wave were destroyed.

What Mr. Bosanquet has to do is to reconcile this state of things with the production, under the same circumstances, of a quadruple amount of illumination or loudness.

The received theory in effect states that where two equal waves in the same phase act together, the effect of each in producing, at a distant point of the air or æther at a given time, effective force, velocity, and displacement will be precisely *the same* as either would have produced under the same circumstances if it had acted separately—and that if we have three or more such waves superposed, no alteration will occur in the separate action, so estimated, of each.

The received definition of intensity, on the other hand, implies that, coexisting with the state of things just described, we shall have, where two waves operate together, *each* wave producing *twice* the effect estimated by the amount of sound resulting from it which either would produce separately ; where three waves act together, *each* will produce *three* times the effect, so estimated, which it would have produced separately ; and so on.

To what purpose, I would ask, are we called upon to embarrass ourselves with these extravagant conclusions ? The definition of intensity was invented for the special purpose of explaining the phenomena of interference, or, to speak more precisely, to determine the points of maximum and minimum intensity in interfering rays ; and whether we take the simple power of the amplitude or its square as the measure of intensity, the positions

of maximum and minimum intensity so resulting will be absolutely identical.

6 New Square, Lincoln's Inn,
    March 15, 1873.

P.S.—Fresnel, in his "Mémoire sur la Diffraction de la Lumière" (*Mémoires de l'Académie*, Paris, 1826, p. 406), distinguishes between "l'intensité des vibrations" as depending on the simple power, and "l'intensité de la lumière" as depending on the square of the particle-velocity. Can any intelligible ground be assigned for this distinction?

XLVI. *On Diffraction.* By G. Quincke[*].

IN a searching investigation of the phenomena which occur in the inflection (diffraction) of light, I have arrived at results which deviate from the representations hitherto given, in several, and, I think, important points.

Some time since (see Pogg. *Ann.* vol. cxlvi. pp. 1–65, 1872), I treated theoretically the phenomena which are perceived when a point or a line of light is looked at, with a telescope or with the naked eye, through a diffraction-grating—that is, a combination of apertures of the same size and shape and at equal distances from each other. The theory therefore comprises gratings with opaque or transparent bars, as well as such as are cut with a diamond-point in a plane glass or metal plate. Besides the validity of Huyghens's principle, it was therein presupposed that a furrow-grating consists of depressions with little stair-like steps, one face of which is parallel to the untouched face of the plate.

For furrow-gratings the formulæ are much more complicated than for gratings with opaque bars. They show, in accordance with the experiments, that the luminous intensity with these gratings (which in practice are used in preference) depends very considerably on the dimensions of the furrows and on the substance with which they are filled up, whether the light be transmitted through the grating or reflected from it.

The investigation of reflected light affords this advantage, that the experiment can be better accommodated to the presuppositions of the calculation than with transmitted light. Symmetrically formed furrows, and elevation-gratings of the same material (which can be very completely produced galvanoplastically with the aid of some experimental artifices), exhibit the same properties when right and left are exchanged.

For the determination of the wave-lengths of light the so-

[*] Translated from a separate impression communicated by the Author.

called *maxima of the second class*, known to Fraunhofer[*], are ordinarily made use of. The greater the wave-length, and the less the distance between two adjacent groups of apertures of the grating, the greater is the distance of these maxima from one another.

Besides these maxima, however, as we learn from experiment, other maxima, less luminous, make their appearance, which I have named *secondary*, and which the theory does not enable us to foresee. If m denotes a whole number, the secondary maxima are situated at $\frac{1}{m}$, $\frac{2}{m}$, &c. of the distance between two neighbouring maxima of the second class, or at the places where a grating with 2, 3, . . . m times the distance between the apertures or furrows would show maxima of the second class. Their situation relative to the maxima of the second class is, with the same grating, the same in transmitted or reflected light for diffraction in the most diverse substances. The incident rays may make any angle we please with the normal to the surface of the grating. Under otherwise like circumstances, however, the value of m may change with the colour.

The gratings were selected as various as possible; the distance between two adjacent groups of apertures varied between 0·2 and 0·0025 millim. The experiments were made upon gratings with opaque bars in air or water, with apertures in an opaque layer of soot, silver-collodion, silver, gold-leaf, or in iodide of silver on a glass plate, and furrow- or ridge-gratings cut in glass or metal.

I have now studied the diffraction of polarized light by these gratings.

If we look at a sodium-flame through a doubly refracting prism and a grating with vertical apertures or furrows, we see two series of flame-images, one over the other, polarized parallel and perpendicular to the principal diffraction-plane. Two flame-images, one above the other, corresponding to the same maximum of the second class, usually appear equally bright. Only in isolated spots, mostly with feebler intensity of light, do any differences appear. If we advance to flame-images of a higher order, sometimes the light polarized parallel, and sometimes that polarized perpendicular to the principal diffraction-plane may predominate.

Similar differences are observed in reflected light; and, indeed, here again furrow- and elevation-gratings of symmetrical form exhibit the same phenomena as soon as right and left are exchanged.

Slight differences in the shape of the apertures, or furrows, or

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[*] Gilbert's *Ann.* vol. lxxiv. p. 340 (1823).

elevations of a grating have a very considerable influence on the difference of intensity of the light polarized parallel and perpendicular to the principal diffraction-plane. The phenomenon changes with the colour of the flame, the substance in which the diffraction takes place, and the angle of incidence of the rays.

Further, in front of the object-glasses of a collimator and an astronomical telescope, I placed two Nicol prisms, the azimuth of which could be determined with accuracy to minutes on vertical circles. The grating was placed between the prisms. In some instances the telescope was laid aside, and the eye looked directly through the analyzing Nicol at the grating. The slit of the collimator was usually illuminated with daylight.

When the Nicol prisms were crossed, the illuminated slit in the eyepiece of the telescope appeared black; on inserting the grating, it was illuminated, and the maxima or spectra of the second class became visible. The central image of the slit appeared variously coloured, according to the position of the Nicol prisms. Viewed with one eyepiece-prism, it mostly shows a dark streak in the spectrum, parallel to the Fraunhofer lines; and on rotating the analyzing Nicol to greater azimuths, with some gratings this streak travels towards the red, with others towards the blue. The latter case, where the component polarized parallel to the principal plane of diffraction is greater for the red than for the blue, is the more frequent.

The amount of rotation of the analyzer which carried the dark streak through the entire spectrum varied with the angle of incidence, the nature of the material of the grating-bars or furrows, the fineness of the grating, and the substance in which the diffraction took place. It varied from a fraction of a minute to $\frac{3}{4}°$ in transmitted light.

In lateral spectra likewise, parallel to Fraunhofer's lines appear dark streaks, which with the rotation to greater azimuths, according to the grating and the spectrum, go from the red to the blue, or from the blue to the red. The rotation is very different with different gratings, and with different lateral spectra with the same grating, and may amount to $5°$ or more. With greater angles of diffraction the superposition of spectra of different orders disturbs the observation.

When several parallel gratings are inserted one behind another, very complicated phenomena enter, which have been partly investigated by Brewster[*] and Crova[†]. With a suitable arrangement of the gratings, the turning of the polarization-plane for a determined maximum of the second class can be increased.

Often the dark streaks do not appear in the spectrum until,

[*] Phil. Mag. S. 4. vol. xxxi. pp. 22 & 98 (1866).
[†] *Comptes Rendus*, vol. lxxii. p. 855 (1871); vol. lxxiv. p. 932 (1872).

simultaneously with the grating, a mica plate of $\frac{\lambda}{4}$ is put between the Nicol prisms in a suitable azimuth. The diffracted light is then elliptically polarized. For individual gratings the difference of phase of the components polarized parallel and perpendicular to the principal diffraction-plane can be determined by a Babinet compensator.

Still more striking than in transmitted light are the phenomena when, in the azimuth $\pm 45°$, linearly polarized light is reflected from a grating, especially a silvered furrow or elevation-grating. In the spectrum of the central image, or the side spectrum of the second class, with the Nicol prisms in a certain position, one or more dark streaks then make their appearance, which when the Nicols are rotated travel from one Fraunhofer's line to another or disappear. Their site varies with the form, distance, and material of the furrows or elevations, the angle of incidence, and the substance in which the diffraction takes place. Furrow- and elevation-gratings of symmetrical form again show the same phenomena. Deviations are to be accounted for by small differences in shape of the furrows or elevations, which very considerably influence the phenomenon.

Between crossed Nicol prisms, a grating shows in the transmitted or reflected light secondary maxima with some Fraunhofer's lines which without them are not perceived. They are variously coloured, according to the azimuth of the analyzing prism.

Different gratings show quantitative, but not qualitative differences, as I have found by numerous measurements, which will soon be given in another place, where the labours of other observers will also be described.

Abstracted from all theoretical considerations, the experiments showed :—

1. Linearly polarized light gives in general, after diffraction, light elliptically polarized.

2. The difference of phase and ratio of amplitude of the components polarized parallel and perpendicular to the principal diffraction-plane vary, the angle of incidence being the same, with the order of the spectrum, so that with an increasing angle of diffraction they may become greater or less. An increase or diminution, however, may be succeeded by a diminution or an increase, and so forth.

3. The increase or diminution is very different for different colours ; and, under otherwise similar conditions, one colour may show an increase, another a diminution.

4. If the difference of phase of the two components polarized parallel and perpendicular to the principal diffraction-plane is small, in the diffracted light a rotation of the plane of polar-

ization is perceived, which with the same angle of incidence and the same spectrum of the second class is different in amount, and the absolute value of which may become greater or less as the wave-length increases. To an azimuth $+\alpha$ or $-\alpha$ of the incident light corresponds after the diffraction the same azimuth $+\beta$ or $-\beta$ of the transmitted or reflected light. For the directly transmitted or reflected rays (corresponding to the diffraction-angle $0°$) the rotation of the plane of polarization may amount to a few minutes or several degrees—with the lateral maxima of the second class, to $90°$ or more. The more frequent case is where the amplitude polarized perpendicular to the principal diffraction-plane, or parallel to the lines (furrows) of the grating, is greater for blue than for red light.

5. A grating inserted between Nicol prisms or polarizing apparatus, when the incident light is white, imparts to the directly transmitted or reflected light similar colours to those shown by plates of crystal between polarizing arrangements.

6. Ratio of amplitude and difference of phase vary, under otherwise like conditions, with the inclination of the grating to the incident rays.

7. Ratio of amplitude and difference of phase change, for normal as well as for oblique incident rays, with the substance of which, with transmitted light the surface of the bars, with reflected light the furrows or elevations of the grating consist.

8. The amplitude-ratio and phase-difference change with the width of the apertures or the form of the furrows or elevations.

9. The finer the grating, or the more it is inclined to the incident rays, the greater, *cæteris paribus*, is the change produced by diffraction in the ratio of amplitude and the difference of phase of the light-waves polarized parallel and perpendicular to the principal plane of diffraction.

10. The light reflected from furrowed metallic mirrors directly in the principal diffraction-plane exhibits very nearly the same difference of phase as with smooth mirrors of the same material. The amplitude polarized parallel to the reflection- or principal diffraction-plane predominates still more over the amplitude polarized perpendicular to the plane of incidence than with unfurrowed metallic mirrors. The direct reflected light from furrowed metallic mirrors approaches nearer in its properties to that reflected from transparent substances than does that which is reflected from smooth unfurrowed metallic mirrors.

11. With gratings in other respects alike, the phenomena vary with the substance in which the diffraction takes place.

12. The secondary maxima, for which the theory does not account, exhibit the same remarkable behaviour towards polarized light as the maxima of the second class.

13. Furrow- and elevation-gratings of symmetrical form exhibit so nearly the same behaviour towards polarized light, if right and left are exchanged, that the phenomena may be regarded as identical.

For the explanation of these phenomena, I think it must be admitted that *difference of phase and ratio of amplitude of the light polarized parallel and perpendicular to the principal diffraction-plane depend on the diffraction-angle and also on the substance and the magnitude of the boundary between the heterogeneous parts of a grating upon which the unit of cross section of the incident light falls.*

This tells in favour of an influence of the molecules of the substance upon the oscillations of the æther particles, and the inadmissibility of Huyghens's principle at the margins of the apertures or furrows of a grating.

The theorems found by experiment for gratings with groups of apertures of like form, at equal distances from one another, must hold also for gratings with similar-formed groups of apertures at unequal distances from each other or for single groups of apertures, and even for heterogeneous particles distributed in a homogeneous base. Therein the distance and magnitude of these particles may be less than a wave-length.

Indeed polarized light exhibits the same behaviour towards single slits and furrows, or towards gratings with like-formed groups of apertures at unequal distances from one another, as, according to the searching investigations of Fizeau*, it shows towards ordinary gratings. Further, similar are the phenomena of the polarization of the light of the sky which Arago†, Babinet‡, and Brewster§ have observed, the polarization pointed out by Govi‖ and Tyndall¶ in clouds of fine particles of dust and vapour, and the polarization of diffused light which occurs when diffraction is produced by very small heterogeneous particles distributed in water or other homogeneous transparent liquids or solids, as described particularly by Soret** and Lallemand††.

All these experiments show that the light polarized parallel to the plane of diffraction may have greater, less, or the same intensity as that which is polarized perpendicular to the same

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* *Comptes Rendus de l'Acad. des Sci.* vol. lii. pp. 267, 1221 (1861).

† *Werke, deutsch von* Hankel, vol. vii. pp. 327, 359 (1824).

‡ *Comptes Rendus,* vol. xi. p. 619 (1840).

§ Ibid. vol. xx. p. 802 (1845), xxiii. p. 234 (1846).

‖ Ibid. vol. li. pp. 360, 669 (1860).

¶ Phil. Trans. 1870, p. 348.

** *Arch. d. Sc. Phys.* vol. xxxv. p. 54 (1869), xxxvii. p. 148, xxxix. p. 1 (1870).

†† *Comptes Rendus,* vol. lxix. pp. 189, 282, 917, 1294 (1869), lxx. p. 182 (1870), lxxv. p. 707 (1872).

plane, and consequently that the direction of the oscillations of the æther relative to the plane of polarization cannot be determined as the theories and theoretical considerations of Stokes*, Holtzmann†, Lorenz‡, Lallemand§, and Strutt ¶ have attempted to determine it, from the behaviour of diffracted light.

XLVII. *On certain Early Logarithmic Tables.*
By Professor D. Bierens de Haan**.

IN reference to a very interesting Note of Mr. J. W. L. Glaisher's, I think I can give additional elucidation on some points. I have now before me the two works of Ezechiel de Decker, the first having the title :—

Eerste deel ‖ van de nievwe ‖ Telkonst, ‖ inhovdende versheyde ‖ manieren van rekenen, waer ‖ door seer licht konnen volbracht worden de Geo- ‖ metrische ende Arith-metische questien. ‖ Eerst ghevonden van Ioanne Nepero Heer ‖ van Merchistoun, ende uyt het Latijn overgheset door ‖ Adrianvm Vlack. ‖ *Waer achter bygevoegt zijn eenige seer lichte manieren van Rekenen*‖ *tot den Coophandel dienstigh, leerende alle ghemeene Rekeninghen* ‖ *sonder ghebrokens af veerdighen. Mitsgaders Nieuwe Tafels* ‖ *van Interesten, noyt voor desen int licht ghegeven.* ‖ Door Ezechiel de Decker, Rekenm^r. ‖ Lant-meter, ende Liefhebber der Mathematische ‖ kunst, residerende ter Goude. ‖ *Noch is hier achter byghevoeght de* Thiende *van* ‖ Symon Stevin *van Brugghe.* ‖ By Govde, ‖ By *Pieter Rammaseyn,* Boeck-verkooper inde corte ‖ Groenendal, int Vergult A B C. 1626. ‖ *Met Previlegie voor thien Iaren.*

Pages xii (not paged) ; A–Rr (page 1–308) ; a–q (128 pages, not paged) ; A–D (pages 1–27), quarto.

The first twelve pages contain, after the title, a copy of the privilege ; a dedication to the States General, to the Councillors of Holland and West Frisia, to the Mayor and Sheriffs of the town of Gouda ; a preface to the benevolent and scientific reader (Voor-reden tot den Goetwilligen ende Konstlievenden Leser); three sets of Latin verses by Patricius Sandaeus and Andreas Junius ; the index (Register van alle de Hoofstucken) ; and the table of errata (de Druck-fauten salmen aldus verbeteren).

The 39 following sheets, A–Rr (the sheet Cc has but four

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* Cambr. Trans. vol. ix. p. 35 (1851).
† Pogg. *Ann.* vol. xcix. p. 446 (1856).
‡ Ibid. vol. cxi. p. 321 (1860).
§ *Comptes Rendus,* vol. lxix. p. 190 (1869).
¶ Phil. Mag. S. 4. vol. xli. p. 450 (1871).
** Communicated by the Author.

pages, where begins a collection of tables for the alloyage of silver and gold), contain by J. Neper :—

Eerste Boeck vande Tellingh door Roetjes, van het gebruyck der Telroetjes intgemeen (Rabdologiae liber I.) . pp. 1–40

Tweede Boeck vande Tellingh door Roetjes, van het gebruyck der Telroetjes in Meetdaden ende Werckdaden met behulp van Tafels (Rabdologiae liber II.) . . . . . pp. 41–88

Aenhangsel van het Veerdigh-ghereetschap van Menighvul-digingh (Appendix de expeditissimo Multiplicationis Promp-tuario) . . . . pp. 89–112

Van de Plaetselicke Telkunst (Arithmetica localis) :

pp. 113–148

And by Ez. de Decker,

Van Coopmans Rekeninghen (Mercantile Arithmetic) :

pp. 149–308

The 16 sheets that follow (a–q) contain Jaer- en Maent-Tafels van Interest teghen 5 (6, 7, 8, $7\frac{9}{13}$, $7\frac{1}{7}$, $6\frac{2}{5}$, $6\frac{1}{4}$) ten hondert. (Discount Tables.)

At the end we find a translation of Simon Stevin's la Disme in 27 pages ; it has a title by itself (and is printed separately ; at least I have a copy quite identical with it).

De || Thiende. || Leerende door || onghehoorde lichtigheyt alle re- || keninghen onder den Menschen noodigh val- || lende, afveerdighen door heele ghetal- || len, sonder ghebrokenen. || Door Simon Stevin || van Brugghe. || Ter Govde, || By Pieter Rammaseyn, Boeck- || vercooper, inde Corte Groenendal, int Duyts || Vergult A B C. || M.DC.XXVI.

De Decker, a surveying engineer, gave lessons in mathematics [Preface : "Terwyl ick inde Vermaerde stadt Gouda Professie doende van de Meetkonst ende Rekenkonst"] ; but he did not understand the Latin language [Pref. : "Doch also ick inde Latijnsche sprake onervaren was"], and therefore had recourse to Adriaen Vlacq, who was at that time bookseller, it seems, under the firm of Pieter Rammaseyn ; for in the copy of the Privilege, which is given to Vlacq himself ["conventeren en accorderen mits desen Adriaen Vlacq"] for ten years, it is decreed that in case of counterfeiting there be forfeited the sum of a hundred and fifty guilders, in order to give a third part to the officer that shall make the challenge, another third to the poor, and the last third to Adriaen Vlacq. This Vlacq, a young man of 26 years, who understood the Latin language, and was at the same time a good mathematician [Preface : "den konstlievenden Ionghman Adriaen Vlac, die hem doenmael met grooten yver inde Meetkonst oeffende"], translated for de Decker the 'Mirifici Logarithmorum Canonis Descriptio' of Joannis Neperi ; but this seemed to take too high a flight for de Decker's purpose. He was better pleased

with the translation of Joannis Neperi Rabdologia, which he gave in the volume just described. In the second volume he purposed to give the translation of Henrici Briggii Arithmetica Logarithmica and the Tables of Edmund Gunterus [Professor in Astronomie tot Londen]; but he cautions the reader to have some patience till that second volume be printed ["verwachten met patientie, tot dat het Tweede Deel voldruckt is"].

Now this second volume has never appeared; and by and by I will give a conjecture why not.

At the same time that the first volume appeared, at least with the same date [4 September int Jaer 1626] he gave another work with the title Nievwe ‖ Telkonst, ‖ inhovdende de ‖ Logarithmi voor de ghe- ‖ tallen beginnende van 1 tot 10000, ghemaeckt ‖ van Henrico Briggio Professor ‖ van de Geometrie tot Oexfort. ‖ Mitsgaders ‖ *De Tafel van Hoeckmaten ende Raecklijnen door* ‖ *het ghebruyck van Logarithmi, de Wortel zijnde van* ‖ 10000,0000 *deelen, gemaeckt van* Edmund. Gun-‖ tero, *Professor vande Astronomie tot Londen.* ‖ Welcke ghetallen eerst ghevonden zijn van ‖ Joanne Nepero Heer van Merchistoun : ‖ Ende ‖ *'t gebruyc daer van is met eenige Arithmeti-* ‖ *sche, Geometrische ende Spherische Exempelen* ‖ *cortelick aenghewesen,* ‖ Door *Ezechiel de Decker,* Rekenmeester, ende ‖ Lantmeter residerende ter Goude. ‖ Ter Govde, ‖ By *Pieter Rammaseyn,* Boeck-verkooper inde ‖ corte Groenendal, int vergult A BC. 1626. ‖ *Met Privilegie voor thien Iaren.*

Pages viii (not paged) ; A–D (pages 1–51), A–M (169 pages not paged), A–F (91 pages not paged, in octavo). The first eight pages contain the title, the privilege (again to the name of Adriaen Vlack, as before), the preface, and three sets of verses by Andreas Junius (different from those in the first volume). In the preface de Decker rejects the logarithms of Neper, and adheres to those of Briggs ["om d'eerste Logarithmi te verwerpen, ende dese aen te nemen"] ; and after speaking of his editing the second volume, that will become greater than the first one ["ende nadien dit grooter sal zijn als het Eerste Deel"], and therefore require some time for printing, announces that he is willing to give a Manual for some amateurs' sake that they may profit thereby in the mean time ["Maer alsoo eenige Liefhebbers verlanghen het gebruyck daer van te moghen sien, ende doch eenighe Handtboeckxkens van doen hebben, soo hebben wy dit doen Drucken"]. For this reason he gives the logarithms from 1 to 10,000, taken from the 'Arithmetica Logarithmica Henrici Briggij,' and a Table for Sines &c. of Edmund Gunter. The first Table occupies the sheets A to M in 169 pages, and has the Latin title :—

Henrici Briggii ‖ Tabvla ‖ Logarithmorvm, ‖ pro nv-

MERIS AB VNITATE, || ad 10000. || GOVDAE, || Typis Petri Ram-
masenij. || M.DC.XXVI. (and not the Dutch one mentioned
by Mr. Glaisher), and gives ten places of decimals with differ-
ences.   The second Table occupies the last sheets, A–F, and
has the Dutch title :—

EDMUNDI GVNTERI || TAFEL || Van Hoeck-maten ende || Raeck-
lijnen, den Wortel zijnde van || 10000,0000 deelen.

These Tables are preceded (pages 1–51) by an Explanation
["Onderwijsingh hoemen de || Tafel HENRICI BRIGGII verstaen ||
sal om te ghebruycken"].

Now Adriaen Vlacq, being the printer of this smaller work,
certainly was already occupied with the calculation of his Tables,
that were published two years later (Gouda, 1628) ; and as these
contained far more than the great work [groote werck] of de
Decker promised or could contain, and as, in the second place, the
last of the books before named was a manual that was very con-
venient for use, I think we might give these two reasons for the
non-appearance of the second volume.   Perhaps, also, logarithms
being still nearly unknown, de Decker may have sold but few
copies of his smaller work (at least it is now very scarce), and
have thought it prudent not to commit his purse by editing the
larger one.   The not selling of this smaller work may have been
the reason that a sufficient number of copies was sent to London,
where they were added to a work of J. Wells, as Mr. Glaisher
has shown.

I think I have shown that these small Tables were printed,
and the English Latin text translated by Vlack, and that de
Decker collected these works in his Nieuwe Telkonst ; so that
it may be surmised that Vlack, after having completed his own
calculations and edited his 'Arithmetica Logarithmica' in 1628,
regarded this, his great work, as completely eclipsing his first
essays (which contained many errors), and so did not allude to
them at all ; for Vlack was not a boasting, but, on the contrary,
a very modest man, as may appear in the title,

ARITHMETIQVE || LOGARITHMETIQVE || OV || LA CONSTRVCTION
ET || VSAGE D'VNE TABLE CONTENANT || les Logarithmes de tous
les Nombres de- || puis l'Vnité jusques à 100000, || ET || D'VNE
AVTRE TABLE EN || laquelle sont comprins les Logarithmes des
Sinus, || Tangentes & Secantes, de tous les Degrez & Minutes
du quart du || Cercle, selon le Raid de 10,00000,00000. parties. ||
PAR LE MOYEN DESQUELLES ON RESOVLT TRES-FACI- || *lement les
Problemes Arithmetiques & Geometriques.* || CES NOMBRES PRE-
MIEREMENT || sont inventez par JEAN NEPER Baron de || Mar-
chiston : Mais Henry Brigs Professeur de la || Geometrie en l'Vni-
versité d'Oxford, les a || changé, & leur Nature, Origine & ||
Vsage illustré selon l'inten-||tion du dit NEPER. || LA DESCRIP-

TION EST TRADVITE DV LATIN EN ‖ *François, la première Table augmentée, & la seconde* ‖ *composée par* Adriaen Vlacq. ‖ DIEV NOVS A DONNÉ L'VSAGE DE LA VIE ET D'EN- ‖ TENDEMENT, PLVS QV'IL N'A FAIT ‖ PAR LE TEMPS PASSÉ. ‖ A GOVDE, ‖ Chez Pierre Rammasein. ‖ M.DC.XXVIII. ‖ *Avec Privilege des Estats Generaux.*

Pages viii. (not paged) contain the title, Preface au Lecteur and Fautes à corriger (which is generally missing); sheets a–g (pages 1–84) contain thirty chapters of explanation; sheets A–Kkk (720 pages not paged) contain Tafel der Logarithmi voor de ghetallen van 1 af tot 100000 (this title, not the French one in the copy at the British Museum, is that occurring in Miller's edition); sheets Lll–Sss (96 pages not paged) contain Canon Triangulorum sive tabula artificialium Sinuum, Tangentium et Secantium, Ad Radium 10,00000,00000, et ad singula scrupula Primi Quadrantis. It should be noticed that every sheet contains 12 pages.

My copy, as appears from a notice on one of the fly-leaves, was changed on March 26, 1667, for the second volume of de Decker's 'Nieuwe Telkonst;' so that it follows again that this latter work was very scarce.

Postscript.—I have found a further proof of the connexion between Vlack and de Decker not having been loosened by the former publishing his 'Arithmetica Logarithmica,' and thereby forcing the latter to suppress his "great work," the second volume of the 'Nieuwe Telkonst.'

The same de Decker gave, some years later, a treatise on Navigation :—

PRACTYCK ‖ vande Groote ‖ ZEE-VAERT : ‖ beschreven door ‖ Ezechiel *de* Decker, *Reker-m*ʳ. ‖ *ende Lant-meter, residerende* ‖ *tot* Rotterdam. ‖ TER GOVDE. By Pieter Rammazeyn, 1631, in octavo (small).

This work contains sheets A–D (the title, dedication, preface, and 60 pages not paged of introductory matter ["Verklaringhen vande Tafels sinuum, tangentium ende secantium; Handelingh vande platte driehoecken; Handelingh van de spherische drye-hoecken]"); sheets 𝔞–𝔣 "Canones Sinuum, Tangentium et Secantium. Ofte Tafel van Hoeck-maten, Raeck-lijnen, ende Snij-lijnen, den Radius zijnde 100000" (92 pages not paged; the tables proceed by 1′ and give 10 decimal-places; as to the logarithms, there is mentioned at the last page the Nieuwe Telkonst in octavo); sheets C–Dd contain some explanations, and then Chapters I. to IX. of the Navigation (pages 1–304, where the last page bears the erroneous superscription "Achste Hooft-stuck"); finally the Almanachs for 1631 to 1640 (pages 305–314), an Appendix [By-Hangsel] with nine Astronomical ques-

tions (pages 305–326), and the Index (pages 327–332, not paged).

Of this work there exists a second, enlarged edition, in 4to, printed at Rotterdam, 1659.

This first edition being printed in 1631 by Vlack himself, the author residing then at Rotterdam, refutes, I think, the idea of Vlack and de Decker having quarrelled; neither is the as-.sumption of a quarrel, as I think I have shown, necessary to explain the non-appearance of the "Groote werck," or the fact that Vlack does not mention de Decker, as the parts of the two volumes of the Nieuwe Telkonst that were translated from Neper, were Vlack's own work and not de Decker's.

Leyden, March 3, 1873.

XLVIII. *On Early Logarithmic Tables, and their Calculators. By* J. W. L. GLAISHER, *B.A., Fellow of Trinity College, Cambridge* *.

THERE are a few remarks in connexion with Prof. de Haan's paper (which he was kind enough to submit to me previously to publication) that it will be convenient to make in immediate juxtaposition with it. And, first, in reference to the paper itself I may recall to mind the circumstances that gave rise to it. My former communications, October and December (Supplement) 1872, contained descriptions of two works which had apparently dropped completely out of notice in regard to the history of logarithms, viz. De Decker's *Nieuwe Telkonst* and *Eerste Deel van de nieuwe Telkonst* (full titles given above) published at Gouda in 1626. The former of these contains logarithms; and in the prefaces of both are acknowledgments of the services rendered by Vlacq in their preparation. The most curious point, however, was, that in both there are allusions to a much larger work on logarithms, for which the reader was directed to wait patiently, using the *Nieuwe Telkonst* merely as a makeshift till its appearance. Now the only book to which these promises (supposing them to have been fulfilled) could apply was the *Arithmetica Logarithmica* of 1628, which, as is well known, was published by Vlacq alone, and contains not the slightest allusion to De Decker, or to any previous publication of logarithms at Gouda. The difficulty, therefore, is to account for Vlacq's total silence with regard to a work in the preparation of which he had had a share, and which contained an announcement either of the *Arithmetica* itself, or of some abandoned project which had been superseded by it. In either case the omission

was equally remarkable; and I expressed an opinion "that Vlacq must have quarrelled with Decker, or for some other cause have had a set purpose to ignore his book entirely." The hypothesis of the quarrel rested on no other evidence except such as the foregoing facts afforded; and I am very glad Prof. de Haan, by showing that friendly relations existed between Vlacq and De Decker in 1631, has proved it to be untenable. He considers the smaller tables were printed by Vlacq, and merely collected by De Decker, and attributes the silence of the former in the *Arithmetica* to the fact that, in comparison with this work, he was rather ashamed of his earlier performances; while De Decker did not keep his word, because Vlacq occupied the ground he had intended to cover. The explanation is not very satisfactory (as, whatever the circumstances may have been, De Decker and the promises should not have been ignored); but it is perhaps the best that will ever be obtained. Prof. de Haan apparently grounds his statement that Vlacq was a bookseller under the firm of Pieter Rammaseyn on the fact that the privileges are made out to him*. At the time of writing my papers I was surprised, though I find that I have not alluded to the matter, at Vlacq's name being mentioned in both the privileges (while neither De Decker's nor Rammaseyn's appear at all in that of the *Nieuwe Telkonst*, where De Decker's alone appears on the titlepage); and I do not even now see the reason very clearly. With regard to the date when Vlacq became a book-seller, his own words are :—" Ignosce quæso, Benevole Lector, si nimis prolixus sum in narranda historiuncula vitæ meæ quam abhinc 26 annis institui : ex eo enim tempore, nescio quo fato, in librorum commercium incidi. In Hollandia, quæ mea patria est, tum temporis vivens, ex Anglia acceperam insignis Mathematici Henrici Briggii *Arithmeticam Logarithmicam*, Opus sanè aureum, et omnium inventionum in artibus Mathematicis meo judicio excellentissimam; in quo erant Logarithmi pro numeris ab unitate ad 20000 et a 90000 ad 100000. Cum-que viderem Logarithmos pro numeris à 20000 ad 90000 ibi desiderari, animus mihi erat lacunam istam explere, quod et in-credibili temporis brevitate solus peregi, ita ut D. Briggius aliique Mathematici in Anglia admirati fuerint, quod non solùm laborem istum, sed et operis promulgationem tam brevi tem-pore absolverim . . . . . . Cum libros istos imprimi curabam, Typographiam nondum habebam, neq; Bibliopola eram, sed

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* Since writing the above I have received a letter from Prof. de Haan, in which, in answer to my inquiry, he says, " I conclude that Vlacq was a bookseller in 1626 from the privilege having been given to him, although he was not the author of the work named on the titlepage. In general these privileges were granted with us to the booksellers themselves.

esse cogebar, ut exemplaria excusa distribuerem; istaq; de occasione primum in Galliam, deinde etiam in Angliam profectus sum, ubi me rem meam bene acturum putabam " (*Joannis Miltoni Defensio secunda*. . . . . *Hag. Com*. 1654. *Typographus pro se-ipso*). These words, taken literally, imply that Vlacq was not connected with the bookselling trade until he began to print the *Arithmetica*, which must have been subsequently to 1626; but I do not think that great importance is to be attached to their exact meaning, as Vlacq here, as in the *Arithmetica*, ignores his early work performed in conjunction with De Decker; and as we know that the *Arithmetica* itself was also printed by Rammaseyn. Thus most likely either Vlacq had nothing to do with printing or bookselling till after the publication of this work in 1628, or he was connected with the trade in some way or other through Rammaseyn from the date of his (or De Decker's) earliest logarithmic work in 1626. The latter view is that adopted by Prof. de Haan, and is very likely correct; but the simple meaning of Vlacq's own words agrees better, I think, with the former. It is proper to state here that a list of names (with dates and places) of editors of logarithmic tables appended to a tract of Prof. de Haan's, " Iets over Logarithmentafels " *, contains that of De Decker (Gouda, 1626) ; but this is, as far as I know, the only place in which any allusion is made to the work. It should also be added that the above paper on De Decker's works was written by Prof. de Haan before he had seen my second communication, " Supplementary Remarks, &c." (Phil. Mag. Dec. Suppl. 1872), which accounts for the partial recapitulation of the translation of a portion of the preface of the *Erste Deel van de Nieuwe Telkonst*. In regard to the life of Vlacq, Mr. W. Barrett Davis drew my attention to the following petition, printed in the Calendar of State Papers, Domestic Series, 1637 :—

" Petition of Richard Whitacres to the same [Archbishop Laud]. One Hooganhuysen, a Dutchman, being heretofore complained of in the High Commission for importing books printed beyond the seas, was bound not to bring in any more. One Vlack has kept up the same agency, and sold books in his stead, and is lurking here, observing what is most useful and vendible, and causes it forthwith to be printed abroad. Petitioner having lately brought from Frankfort mart to Rotterdam four great vats of books to the value of £500, Hooganhuysen,

* Amsterdam, 1862, reprinted from the *Verslagen en Mededeelingen der Koninklijke Akademie van Wetenschappen, Afdeeling Natuurkunde*, Deel xiv. This pamphlet, which I had not met with till Prof. de Haan recently sent me a copy, contains far the most complete list of writers on logarithms or publishers of logarithmic tables that I know of.

upon untrue suggestions, caused them to be seized and sold to
Vlack for £100. Vlack is now preparing to go beyond seas to
avoid answering his late bringing over nine bales of books con-
trary to the decree of the Star Chamber, and procures some
persons to pretend that he is indebted to them (as formerly
Hooganhuysen did), thereby to get the books into their posses-
sion. Petitioner prays order to bring the bales to Stationers'
Hall, there to remain till Vlack shall re-deliver to him the said
four vats of books, or at least at the same price he bought them.
Reference to Sir John Lambe to take three Commissioners'
hands, and by warrant bring the books above-mentioned to
Stationers' Hall, till the cause may be heard. 13th November,
1637 " (Vol. ccclxxi. p. 94).

This series of the Calendar of State Papers is not as yet pub-
lished beyond the beginning of 1639, so that it is not certain
whether Vlacq's name will occur again ; but we know, from his
own account (Phil. Mag. Oct. 1872), that a compromise was
effected. The case of Hooganhuysen (David van Hooganhosen
as he generally appears) was repeatedly before the court for more
than a year. He originally had James Bleau as his co-defendant ;
but the latter was discharged on June 26, 1634, and judgment
against the former given on June 15, 1635, in the following
words :—" Considering the ill-consequence and scandal that
would arise by strangers importing and venting in this kingdom
books printed beyond seas, it was ordered that Hooganhosen
should not bring over or sell Mercator's Atlas or Atlas Major in
English, and if such be brought over by any one they are to be
seized." He was also subsequently (July 7, 1635) ordered to
carry out his contract with regard to the delivery of Amesius
upon the Psalms and other works to certain booksellers. It
will be seen how well the quotation about Vlacq agrees with
his own account. He was about ten years in London, and
finally left on the breaking out of the civil war ; so that he spent
the years from 1632–1642 in England[*], the first few very
peaceably, but afterwards he was treated as he describes. The
petition of Richard Whitacres was very likely the commence-
ment of hostilities.

Of the books which Vlacq names as having been printed by
himself between 1642 and 1648 at Paris, I have not been suc-
cessful in seeing a copy that satisfies these conditions ; but I

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[*] I take this opportunity of correcting a mistake of a name in my first
communication (Oct. 1872). Dr. Johnston should be the well-known
Dr. Juxon, who became Bishop of London in 1633, and as Archbishop of
Canterbury in 1660 crowned Charles II. The Latin is *Joxstonius*, which,
considering the x a misprint for n (a not uncommon interchange), I ren-
dered Johnston. Vlacq, writing after an interval of fifteen years, must have
only partially remembered the name.

have met with an edition of one of them, viz. *Hugonis Grotii de Imperio summarum Potestatum circa sacra Commentarius,* editio quarta. . . . printed by Vlacq at the Hague in 1651 ; the *De Jure Plebis* of David Blondèl is added, also printed by Vlacq, with date 1652.

In the Calendar of State Papers (Domestic Series), 1600–1638, Briggs's name occurs several times. He was a Commissioner of Sewers for Norfolk, Suffolk, Cambridgeshire, &c., and in conjunction with Sir Anthony Thomas, John Worsopp, Hildedebrand Prusen, and others was an undertaker for draining the fens (Dec. 2, 1629). When in February 1625 the tides over-threw 1120 rods of bank in the neighbourhood of Yarmouth, Briggs was consulted with regard to the levels. He was also a member of a commission to effect the removal of several houses in Oxford ; and his death is reported by the Vice-Chancellor (Jan. 27, 1631) as having occurred the previous day.

Under date Nov. 12, 1630, there is, in the handwriting of Archbishop Laud, a list of Master Printers of London, with a sum placed against each, headed " To S. Paul's " (viz. to the repair of St. Paul's). The Calendar states that the sums assessed run from £6 to George Miller, to £40 to William Jones. This is the only mention I have anywhere seen of the George Miller who printed the English (1631) edition of Vlacq (except the *tables* themselves, which were all printed at Gouda) ; William Jones was the printer of Briggs's *Arithmetica* of 1624.

In more than one place I have remarked that a good many of the English (Miller) copies of the *Arithmetica* contain the titlepages to the Tables in Dutch, and have inferred therefrom that Vlacq either did publish, or meditated the publication of a Dutch edition of his work (that is to say, of the Introduction to it ; the tabular portion is the same in all editions) ; and this view is confirmed by the following extract from a letter of Briggs to Pell (MS. Birch, orig. 3495, now made 3498*). " My

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* The letter is addressed " To his very good and muche respected frende Mr. John Pell at Trinitie Coll. in Cambridge," and is sealed with what was, I suppose, Briggs's seal. The device may be thus described :— Draw a parabola with axis vertical upon a horizontal line terminated both ways by the curve as base. Divide this base into seven equal parts, and erect six ordinates at the points of section, terminated by the curve. Join one extremity of the base to the top of the third ordinate from the other extremity, and the top of the first ordinate to the top of the second ordinate from the other extremity, and render the figure symmetrical by joining the other corresponding points. Then, supposing these four lines passed through a point (in point of fact they do not), we should have a representation of the device, the motto under which is " IN VNVM." What the proposition referred to is, I do not know. I at first thought it was as described above, but found on investigation that, although in a drawing the lines pass well enough through a point, they do not really do so.

desire was to have those Chiliades that are wantinge betwixt 20
and 90 calculated and printed, and I had done them all almost
by my selfe, and by some frendes whom my rules had sufficiently
informed, and by agreement the busines was conveniently parted
amongst us; but I am eased of that charge and care by one
Adrian Vlacque, an Hollander, who hathe done all the whole
hundred chiliades and printed them in Latin, Dutche, and
Frenche, 1000 bookes in these 3 languages, and hathe sould
them almost all.   But he hathe cutt of 4 of my figures through-
out; and hathe left out my Dedication, and to the reader, and
two chapters the 12 and 13, in the rest he hathe not varied from
me at all." (The whole letter is printed in the ' Letters on
Scientific Subjects ' published by the Historical Society of
Science in 1841, under the editorship of Mr. Halliwell.)

I wrote and asked Prof. de Haan if he knew of, or could find,
any Dutch copy in the Libraries at Leyden; and he has in-
formed me he has not been able to meet with any trace of any
such having appeared.   I think it likely that the copies origi-
nally intended for the Dutch edition were, when it appeared that
there was little demand for them in Holland, sent to London to
be appended to an English introduction, and sold over here.
Whether George Miller bought the copies, or merely printed
the Introduction for Vlacq is uncertain; but the latter supposi-
tion is in accord with Vlacq's having come to London in 1632
(the year of publication) as a bookseller.

No one can fail to notice the total absence of any feeling of
irritation on Briggs's part against Vlacq for having anticipated
him in the performance of a work he had so much at heart, and
which he had nearly performed himself.   This is in perfect
agreement with every thing else that is known of Briggs; and
there is no reason to doubt that he stated the simple truth when
he closed his preface to Wright's translation of the *Canon Miri-
ficus* with the words, " I ever rest a lover of all them that love
the Mathematickes."   Who Briggs's friends were who were
helping him with the calculation can only be conjectured.   Very
likely Gunter may have been one; and there is reason to think
that J. Welles, of Deptford, the author of the *Sciographia*, was
another; for in a letter from the latter to Briggs, dated January
9, 1621, and printed in the ' Correspondence of Scientific Men
of the Seventeenth Century, in the Collection of the Earl of
Macclesfield,' 1841–1862, he speaks about the calculation of
logarithms in a manner which implies that he was assisting
Briggs in the computations needed for the *Arithmetica*, 1624.   In
reference to this work it is worth while to quote a sentence that
occurs in a letter from Collins to Wallis under date Feb. 2, 1668
(in the same collection) :—" Mr. Briggs's *Arithmetica Logarith-*

mica being too numerous an impression, has been tendered about the streets at 1*s.* 6*d.* each. The like I say of Mr. Barrow's Euclid. Mr. Sutton and myself, as Mr. Marke well knows, have bought divers of them at 1*s.* a book, in quires." There is very little about the calculators of logarithms or their works in the Macclesfield collection; but it appears that in 1673 Flamsteed borrowed a copy of Vlacq's *Trigonometria Artificialis* from Collins. In a letter to Thomas Lydyat (July 11, 1623) printed in the Letters of the Historical Society of Science, Briggs speaks of being engaged on his logarithms; but it is sufficiently evident from other considerations that he must have been so employed at the time.

Birch, 4407, contains a MS. entitled "Imitatio Nepeirea. Sive Applicatio omnium (fere) regularum suis Logarithmis pertinentium ad Logarithmos M^{ri} Brigges. Ex propriâ* descriptione sui Canonis Mirifici. Impressâ Edinburgi Anno 1614." The tract, which is not in Briggs's handwriting, I should imagine, from internal evidence (though I have made no very careful examination of its contents), to have been written about 1623. It seems to be (as its name implies) a sort of parody, with Briggian logarithms, of Napier's *Canon Mirificus*; but does not appear to possess any particular value or interest.

April 16, 1873.

XLIX. *Notices respecting New Books.*

Celestial Objects for Common Telescopes. By the Rev. T. W. WEBB. London: Longmans.

Report presented to the Board of Visitors of the Royal Observatory, Edinburgh. By C. PIAZZI SMYTH, *Astronomer Royal for Scotland.*

THE appearance of a third edition of the valuable epitome of practical amateur astronomy first mentioned is a strong evidence of the healthy condition of the science, considered apart from the ordinary work of established observatories. It is now some thirty years since an impetus was given to the labours of the amateur by the publication of the late Admiral Smyth's 'Celestial Cycle:' and there can be no doubt that the progress made in a knowledge of double and binary stars between its publication and the year 1859 is mainly attributable to the 'Bedford Catalogue,' which formed the second part of the 'Cycle,' and for which the author received the gold medal of the Royal Astronomical Society. Originally dedicated to Admiral Smyth, the work which forms the subject of this notice, in addition to most valuable information relative to the sun, planets, and comets, embodies in a condensed form the most important characteristics of double stars, clusters, and nebulæ; and we have only to refer to the edition which has just appeared to

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* The word which I take to be *propriâ* is a correction in a later hand of that originally written, which was perhaps *ejus.*

become acquainted with the fact that this condensed catalogue has been brought up to the date of publication by the industrious author, and thus supplies the hiatus created by the 'Bedford Catalogue' being out of print; indeed the 'Bedford' is now becoming a record of the past, exceedingly valuable as showing the state of this branch of astronomy in 1844, but inadequate to the wants of the working amateur in 1873.

The first edition of the 'Celestial Objects' was marked by a feature almost unique, the only parallel being Tables of Lunar objects (mostly from Schröter) in the late Sir David Brewster's Supplementary Chapters to Ferguson's 'Astronomy,' published in 1821, accompanied by a map of the moon by Mayer, a predecessor of Schröter. Mr. Webb embellished his first edition with an admirable lunar map from Beer and Mädler's *Mappa Selenographica*, which for clearness of detail and facility of reference can hardly be surpassed, and gave the names of the 404 objects which are found on the German map, accompanied with short descriptive notices of the most important. It was the publication of this map and catalogue which drew that attention to the study of the moon's surface which has resulted in a revised impression of the map, containing ninety additional numbers referring to as many new names in the list. An appendix includes the latest information bearing on telescopes, observations of the Sun, Venus, the Moon, Jupiter, Comets, and the so-called fixed stars.

We apprehend that no amateur astronomer would willingly be without this valuable compendium of his science.

Turning from Amateur to Government work, we regret to find, from the Report of the Astronomer Royal for Scotland, that nothing additional is to be given to the Royal Observatory, Edinburgh. It appears that an extensive task is projected—that of forming a general catalogue of Edinburgh stars from the earliest days of the activity of the Observatory. The personal staff consists of the Astronomer Royal for Scotland and two assistants only, the daily duties being the computation of meteorological observations from fifty-five stations of the Meteorological Society of Scotland, the observation of stars for time, and its distribution electrically by time-ball, time-gun, and controlled clocks. A new time-gun has been established in Dundee, and a new controlled clock added to the primary series of the Observatory, the gift to the Edinburgh University of a private gentleman of liberal mind and intelligent interest in science. It is principally in connexion with the new equatorial, now nearly complete, that the Astronomer Royal for Scotland deplores the want of further aid from Government. Speaking of the pressing difficulties arising from want of funds, he says that the determination of Government places him in the position of an unfortunate artillery officer who should have received a big gun, of perhaps the most approved wrought iron and steel construction in itself, but without the means of moving it, without powder and shot, and yet should be expected by the public to be continually firing it with immense success at all sorts of objects throughout the whole year.

L. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 311.]

March 6, 1873.—Sir George Biddell Airy, K.C.B., President, in the Chair.

THE following communication was read :—
"On the Vapour-density of Potassium."—Preliminary Notice. By James Dewar and William Dittmar.

Since the elaborate experiments of Deville and Troost on the vapour-densities of substances at high temperatures, little has been added to chemical science in this field of research. Doubtless this is in great part owing to the difficulty of any *one* student manipulating the complex apparatus necessary for the execution of the experiments. But the operations are greatly increased in difficulty when we select bodies that are readily inflammable in air and attack with facility glass and porcelain at the high temperatures to which they are exposed. This is the reason why the molecular weights of a most important class of elementary bodies, viz. the *alkali-metals* (although these are volatile at moderate temperatures), have remained to the present time undetermined. It was with the view of adding something to our knowledge in this department, that we recently undertook some experiments with potassium, the results of which we now beg leave to lay before the Society. The special difficulties we had to overcome are involved in the endeavour to answer the following questions :—

1. Is it possible to convert potassium into a gas of one atmosphere's pressure at any of the *constant* temperatures we can at present command ?

2. Is it possible to generate *pure* potassium-vapour and to keep it from getting oxidized ?

3. Supposing a definite volume of such vapour to have been procured, how can its *weight* be ascertained ?

After a succession of failures, which we shall not detail, we at last succeeded in devising a workable process, which may be briefly described as follows :—

A cylindrical iron bottle of at least 200 cub. centims. capacity, of a thickness in the body ensuring sufficient rigidity at even a bright red heat, and provided with a well-ground inbent neck, pierced with a canal of about 2 millims. diameter, is employed as a generator and receptacle of the vapour.

A mass of about 20 kilogrs. of zinc contained in a plumbago crucible, which being placed in a forge-fire can be readily heated up to the boiling-point, serves as a bath.

The experiment begins by first deoxidizing the inside of the receptacle at a red heat by means of a current of dry hydrogen, which is continuously maintained until the bottle has cooled down below redness. At this stage about 200 grms. of pure mercury are introduced into the bottle, which is then inserted into the red-hot zinc, without, however, covering the upper extremity of the

bottle. After $\frac{3}{4}$ of the mercury is distilled off (which is accomplished in a very short time), the neck is withdrawn; and while the mercury-vapours are still streaming out, an iron test-tube, previously prepared with great care and charged with 4–5 grms. of potassium, is dropped into the bottle, the neck reinserted, and, after the *whole* of the bottle has been immersed in the zinc, the blast of the forge is forcibly increased so as, in the shortest possible time, to bring the zinc into the state of boiling, proper arrangements being made for keeping the neck of the bottle red-hot. The potassium in a short time begins to volatilize, issuing in jets into the air and depositing caustic potash at the nozzle, which must be kept clear by means of an iron wire. As soon as the distillation of the potassium ceases, the nozzle is closed by means of a ground-in wire plug, at once immersed into a mass of mercury contained in a test-tube, and the bottle withdrawn to a proper support, on which it is allowed to cool.

After it has reached a manageable temperature, the bottle is inserted into a mass of recently boiled water, the wire plug withdrawn, and the hydrogen formed by the action of the water on the potassium pumped out, by means of a "Sprengel," into a eudiometer, to be measured.

In the experiments we have hitherto carried out, we have satisfied ourselves that the amount of mercury-vapour *not* swept out by the potassium is quite inappreciable; and as our object has been in the mean time to merely arrive at approximate results and to perfect our methods of manipulation, we have neglected the minute correction which, on account of that small remnant of mercury, ought, strictly speaking, to have been applied to the volume of the vapour as calculated from the capacity of the bottle in the cold, the coefficient of expansion of iron, and the temperature ($1040°$ Deville) at which the vapour was measured.

The results of our observations conclusively show that the density of potassium-vapour, as produced in the process described, cannot exceed 45 times that of hydrogen, and that therefore the molecule of potassium consists of *two atoms* (K_2).

We intend to prosecute our research in other directions, proposing to ascertain, if possible, the densities of the *iodides* of cæsium, rubidium, and potassium, these being, according to Bunsen's experiments, the most volatile of the haloids of the alkali-metals.

March 13.—William Spottiswoode, M.A., Treasurer and Vice-President, in the Chair.

The following communication was read :—

"Note on Supersaturated Saline Solutions." By Charles Tomlinson, F.R.S.

In the year 1866, M. Gernez and M. Viollette published each a memoir on supersaturated saline solutions*, in which the same

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* Annales Scientifiques de l'École Normale Supérieure, tome 3ᵉ, Année 1866, pp. 167 and 205. I am indebted for this reference to the courtesy of M. l'Abbé Moigno.

conclusions are arrived at, namely :—(1) that the only nucleus capable of suddenly crystallizing any one of such solutions is a salt of the same kind as that dissolved ; and (2) that all bodies, solid, liquid, or aëriform, which apparently act as nuclei, are really contaminated with a hydrate of the salt that forms the supersaturated solution.

I cannot refrain from expressing my admiration at the unwearied skill and patience with which these two memoirs were prepared. The experiments were repeated by hundreds, and under a large variety of circumstances, so that it seems scarcely possible to entertain any doubt as to the validity of the conclusions arrived at. I had not seen these memoirs until long after the publication of my second paper * ; or I should have hesitated in offering it to the Royal Society without special reference to them. What I did see was a very brief abstract of M. Gernez's memoir in the ' Comptes Rendus ;' and to this I refer in my first paper †, quoting the experiments and the decisive objections of M. Jeannel in opposition to M. Gernez's conclusions.

The experiments of Mr. Liversidge ‡ are identical in principle with those of M. Viollette, and some of them mere variations, such as the proof that bodies greedy of water and capable of being hydrated do not produce crystallization ; only M. Viollette made use of calcined sulphate of copper instead of calcic chloride &c.§ Moreover no fresh proofs are wanted as to the non-nuclear action of the modified salt or of the anhydrous salt on supersaturated solutions of Glauber's salt.

Recently MM. Gernez‖ and Viollette ¶ have each, on the occasion of the publication of my third paper (namely, that written in conjunction with M. Van der Mensbrugghe **), called in question the integrity of my experiments, M. Gernez insisting that the oils and other liquids employed by me contain salts of the same kind as those of the solutions they apparently acted on.

Such an assertion as this seems to me to be very difficult of proof ; and it seems equally bold to assume that the air of an open garden in the country contains salts of various kinds, watching their opportunity to get into my flasks and vitiate the results of my experiments.

I desire to invite special attention to a statement made in my second paper—the more so because, without desiring for a moment to call in question the negative results obtained by MM. Gernez

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* Phil. Trans. 1871, p. 51.
† Phil. Trans. 1868, p. 660. The reference to M. Jeannel is in the abstract of this paper contained in the ' Proceedings of the Royal Society,' May 28, 1868, p. 405.
‡ Proceedings of the Royal Society, vol. xx. p. 497.
§ On the surface-tension theory, it is not inaccurate to say that absolute alcohol robs the solution of water, and so determines crystallization, since the mixture of alcohol and water degrades the tension of the solution.
‖ Comptes Rendus, vol. lxxv. p. 1705.   ¶ *Ibid.* vol. lxxvi. p. 171.
** Proceedings of the Royal Society, vol. xx. p. 342.

and Viollette, namely that pure oils &c., as used by them, whether in the form of films or globules, do not cause the solutions to crystallize, the method indicated opens a new process for ascertaining whether there is really any other nucleus except a salt of the same kind. The statement referred to is as follows :—

" A solution of two parts of Glauber's salt to one part of water was boiled and filtered into three flasks, which were covered with watch-glasses and left until the next day. A drop of castor-oil was then placed upon the surface of each : it formed a lens which gradually flattened ; but there was no separation of salt, even when the flasks were shaken so as to break up the oil into small globules. . . . . If, while the flask is being turned round, a sudden jerk be given to it, so as to flatten some of the globules against the side into films, the whole solution instantly becomes solid" *.

I desire on the present occasion to describe a number of experiments in which this process has been observed. But anticipating the objection that the various oils that have been long on my shelves, and already used in former experiments, may really contain various kinds of saline nuclei, I procured from a wholesale house a number of fresh specimens of oil. These were the oils of lavender, bergamot, cajuput, sesame, rape, almonds, olive, and sperm. I did not allow these oils once to enter my laboratory, but kept them in my library, and never opened the phials except in the open air.

The flasks and dropping-tubes were washed in strong sulphuric acid, and rinsed in tap-water, the tubes being kept immersed in clean water in the open air. The solutions were filtered into the flasks ; and these, being covered with small beakers, were reboiled until steam issued from the orifice.

The flasks were then taken into my garden, and when cold an oil was dropped upon the surface of each solution. If the oil formed a well-shaped lens, there was no separation of salt ; and in many cases, instead of restoring the small beaker to its place, a well-fitting cork was driven into the neck, and the flasks were thus left for a time, on some occasions extending to the next day.

In order to avoid the objection that during the shaking crystallization might be produced by some of the solution splashing against the cork, many of these experiments were repeated in a pear-shaped flask, nearly 12 inches in height, and containing about 2 or 3 ounces of the solution. Most of the experiments were tried in globular flasks of 5 ounces capacity, with straight cylindrical necks 4 inches high.

Experiment 1. Sodic sulphate—1 salt, 1 water. Solution in tall flask covered with a small beaker. Next day oil of sweet almonds was dropped into it, and the flask corked. After about an hour a circular motion was given to the flask, so as to disperse the oil into minute globules through the solution, giving it the appearance of an emulsion. The flask was left at rest during about an hour, then suddenly shaken, so as to rattle the solution against the side, when all at once, as if with a flash, it became solid.

* Phil. Trans. 1871, p. 55.

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Now in this case it may be objected that a crystal of the sodic sulphate hydrate was derived from one of the following sources :— (1) from the air, (2) from the oil, (3) from the cork, or (4) from the side of the flask. (1) It could not be derived from the air, either of the laboratory or of the garden, because the solution remained liquid long after the flask had been corked. (2) It could not have been derived from the oil, because this was dispersed through the solution in myriads of globules, without any nuclear action, and the flask was left to repose for an hour after the oil had been so dispersed. (3) Nor could any nucleus have been derived from the cork, because the solution never touched it. Nor could a minute speck of the sodic sulphate hydrate have fallen from the cork; for the latter had been put into hot water, out of which it was taken the moment it was put into the flask. It could not have been derived from any of the hot water from the cork streaming down the side of the flask to the solution, because sodic sulphate in solution is in the non-nuclear anhydrous state, and also because two hours had elapsed between the corking of the flask and the solidification of the solution. (4) A crystal could not have been derived from the walls of the flask, because the solution had been briskly boiled in it, so that steam escaped with considerable force from the neck after the small beaker had been put on. Hence I am compelled to fall back on my former statement *—namely, that the oil acted as a nucleus by the flattening of one or more of the globules against the wall of the flask into the form of film.

A globular flask, containing the same solution and the same oil, was corked, and the oil dispersed in globules. It was left some hours, with occasional shaking, so as to rattle the solution against the side. The rattling motion was purposely less energetic than in the former case, to avoid splashing against the cork. This solution crystallized after it had been left a short time at rest ; and it did so in large flat crystals, described in my second paper, very different from the minute radial action noticed when crystallization · sets in from a point†.

A similar flask, containing the same solution, was treated with oil of bergamot, a drop of which was allowed to trickle down the side of the flask. As soon as the oil touched the solution, crystallization set in, the radiant-point coinciding with such point of contact, and the whole surface was covered with fine lines diverging from this point alone.

In another case a drop of the oil of rosemary was deposited on the centre of the solution. On shaking the flask, the solution assumed the solid state.

Two flasks, containing the same solution, crystallized under the influence of oil of lavender.

Five flasks, with the same solution, crystallized under the action of sperm-oil.

Experiment 2. Sodic sulphate—2 salt, 1 water. In three flasks

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* Phil. Trans. 1871, p. 52 *et seq.*   † *Ibid.* p. 54.

the solution crystallized on the addition of olive-oil, the first immediately, and the other two after being shaken.

Experiment 3. Sodic sulphate—3 salt, 1 water. This solution crystallized under the action of the oils of bergamot, almonds, colza, rape, and sesame.

Experiment 4. Sodic sulphate—3 salt, 1½ water, in six flasks, all of which crystallized under the action of sesame oil.

Experiment 5. Potash-alum — 8 salt, 3 water. The solution crystallized suddenly, as with a flash, on being shaken up with oil of olives. This is the more remarkable, as the usual action of a nucleus is to produce a solitary octahedron, which grows rapidly until the solution becomes solid. This last effect was produced by a less violent shaking of the solution in contact with oil of lavender.

Experiment 6. Ammonia-alum—8 salt, 7 water. On shaking the solution with olive-oil it suddenly became solid, and of an opaque chalky white.

This selection from a large number of experiments may be sufficient to answer my present purpose—namely, to show that some supersaturated saline solutions really do crystallize under the action of other nuclei than a salt of the same kind as that of the solution operated on.

My experiments were performed in the open air, at temperatures between 30° and 50° F. At comparatively low temperatures the shaking of the flask containing the solution sometimes produced a copious liberation of anhydrous salt, which rapidly combines with water and forms the 7-atom hydrate [*]; but on allowing the flask to rest for some time, it generally happens that the solution becomes solid, and the 7-atom salt opaque white.

I may perhaps be allowed to state that my experiments, conducted as they were in the open air, were delayed by the rainy weather of last year. There is, however, this advantage in wet weather, that the saline particles said to exist in the air are washed down and brought into solution, in which condition they are not nuclear, as I have already shown in the case of sodic sulphate, alum,

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[*] The view adopted by me that the supersaturated solution of sodic sulphate contains the anhydrous salt in solution, and that this is first thrown down on lowering the temperature, agitating, &c., has been objected to, on the ground that, according to Löwel, it is the 7-atom hydrate that is really in solution and is deposited on cooling. There are numerous proofs that it is the anhydrous salt which is really in solution ; these I have collected in two papers, contained in the 'Chemical News' of 3rd and 10th December 1869. That the 7-atom hydrate is built up on the anhydrous salt may, I think, be shown by an experiment. Two flasks containing a solidified solution of sodic sulphate of the same strength are heated over a spirit-lamp ; one of the flasks is constantly turned round on the ring of the retort-stand until the whole of the salt has entered into solution ; it is then boiled, closed, and set aside. The other flask, during the heating, is allowed to remain at rest over the flame for a short time, so as to liberate a portion of the anhydrous salt. This flask is also boiled, although the operation is interrupted by violent bumpings. It is closed and placed by the side of the other flask, under the same conditions. When both flasks are cooled down to the temperature of the air (say about 50°), the flask containing the anhydrous salt will contain a crop of the 7-atom hydrate, built upon the anhydrous deposit. The other flask will have no crystalline deposit at all.

and one or two other salts*. On the other hand, fine weather has its advantages; not only are the surfaces of the solutions more active and the evaporative force stronger, but hydrated salts, said to exist in the air, part with their water of crystallization so readily as to reduce them to the non-nuclear condition. This is especially the case with sodic sulphate. I have already shown that supersaturated solutions of this salt may be exposed to the air, both in fine and wet weather, for a long time without crystallizing †, as well as the fact just noticed ‡, that a solution of a salt does not act as a nucleus to its supersaturated solution. Moreover, if sodic sulphate exist in the dusty air of a room, it cannot retain the hydrated form during many minutes, but must rapidly pass into the anhydrous, in which it is no longer a nucleus.

Seeing, then, that in the case of sodic sulphate, which is said to be always present in the air of rooms, and, according to MM. Gernez and Violette, even in that of the country, the chances are that it is most likely to be present either in the effloresced condition or in solution, and equally non-nuclear in both, I cannot help thinking that too much importance has been given to this part of the subject; for if it be true, we are reduced to the dilemma, pointed out by M. Jeannel §, that there must be floating in the air specimens of all kinds of salts that form supersaturated solutions and crystallize by the introduction of a solid nucleus, whereas there are some such salts which cannot exist in the presence of the oxygen or of the ammonia of the air.

March 27.—Sir George Biddell Airy, K.C.B., President, in the Chair.

The Bakerian Lecture.—"On the Radiation of Heat from the Moon, the Law of its Absorption by our Atmosphere, and its variation in amount with her Phases." By the Earl of Rosse, D.C.L., F.R.S., &c.

In this paper is given an account of a series of observations made in the Observatory of Birr Castle, in further prosecution of a shorter and less carefully conducted investigation, as regards many details, which forms the subject of two former communications ‖ to the Royal Society.

The observations were first corrected for change of the moon's distance from the place of observation and change of phase during the continuance of each night's work; and thus a curve, whose ordinates represented the scale-readings (corrected) and whose abscissæ represented the corresponding altitudes, was obtained for each night's work. By combining all these, a single curve and table for reducing all the observations to the same zenith-distance was obtained, which proved to be nearly, but not quite, the same as

* Chemical News, February 4, 1870, p. 52.
† Proceedings of the Royal Society, 1871, p. 41.
‡ Chemical News, February 4, 1870, p. 52.
§ Ann. de Ch. et de Ph. 4th ser. vol. vi. p. 166.
‖ Proceedings of the Royal Society, vol. xvii. p. 436, vol. xix. p. 9.

that found by Professor Seidel for the light of the stars. By employing the table thus deduced, and also reducing the heat-determinations obtained on the various nights for change of distance of the sun, a more accurate phase-curve was deduced, indicating a more rapid increase of the radiant heat on approaching full moon than was given by the formula previously employed, but still not so much as Professor Zöllner's gives for the moon's light.

By employing Laplace's formula for the extinction of light in our atmosphere, the heat-effect in terms of the scale-readings was deduced, and an approximation to the height of the atmosphere attempted.

From a series of simultaneous measurements of the moon's heat and light at intervals during the partial eclipse of November 14, 1872, when clouds did not interfere, it was found that the heat and light diminish nearly if not quite proportionally, the minimum for both occurring at or very near the middle of the eclipse, when they were reduced to about half what they were before and after contact with the penumbra.

GEOLOGICAL SOCIETY.

[Continued from p. 314.]

November 20, 1872.—Prof. P. Martin Duncan, F.R.S., V.P., in the Chair.

The following communications were read:—

1. " On the Geology of the Thunder-Bay and Shabendowan Mining Districts on the North Shore of Lake Superior." By H. Alleyne Nicholson, M.D., F.G.S., &c.

The author described the general characters of Thunder Bay, which is almost landlocked on the south-east by the bold promontory of Thunder Cape and a series of islands which form a continuation of this. The rocks immediately surrounding Thunder Bay belong to the "Lower and Upper Copper-bearing series" of Canadian geologists. The latter, consisting of sandstones, shales, limestones, marls, and conglomerates, chiefly of a red or reddish colour, with interstratified traps, is regarded by the author as probably of Lower-Silurian age, in accordance with the opinion of Sir Wm. Logan. The "Lower Copper-bearing series" is also very varied in character; it is traversed by trap dykes, and contains several well-marked interstratified traps. It is penetrated by two sets of mineral veins, containing great abundance of silver. The majority of these run along the strike of the beds in a general E.N.E. and W.S.W. direction; the remainder are transverse, running nearly N. and S. Of the latter the most important is the "Silver-islet vein," which is 3 or 4 feet in width, and consists of quartz with native silver and galena; picked specimens of the stuff have assayed from £1000 to £2000 per ton. This vein has been worked for about two years, and has proved remarkably productive. Of the former series the most important is the "Shuniah vein," which runs along at a dis-

tance of 1½ to 2 miles from the north shore of Thunder Bay, the mines in which, although quite in their infancy, promise excellent results. Its wid this 22 feet; and the vein-stuff consists mainly of calc-spar. The silver is present in the native form and as sulphide. The vein traverses hard black shales, but does not run exactly along the strike of the beds; it may be traced for several miles towards the east.

The country between Thunder Bay and Lake Shabendowan, along the "Dawson Road," is of an undulating character; and the surface of its fundamental rocks everywhere exhibits unmistakable evidences of glaciation, the general direction of the striæ being N. and S., but with the occasional occurrence of a minor set of grooves running nearly E. and W. The greater part of the country is thickly covered with drift, composed of rocks which appear to have travelled from north to south.

The rocks passed over between Thunder Bay and Lake Shabendowan are described by the author as, 1, the shales and traps of the "Lower Copper-bearing series;" 2, a range of syenitic and gneissic rocks, probably of Laurentian age; 3, a great series of rocks belonging to the Huronian group, consisting of greenish or grey slates, with bands of gneiss and trap dykes, and bedded green traps with great masses of greenish, grey, or drab-coloured slates, the whole presenting a close resemblance to the green slates and porphyries of the English Lake district. The slates, in the author's opinion, are bedded felspathic ashes.

The author described the general characters of Lake Shabendowan, and stated that from the foot of the Lake for about 15 miles westward there is a succession of trappean rocks, beyond which, to the head of the lake, distant 13 miles, the country is occupied by Huronian slates like those between the lake and Thunder Bay. These slates extend for an unknown distance north-west of the head of the lake, and contain numerous veins, having an E.N.E. and W.S.W. direction, conformable with the strike of the beds; and some of them are auriferous. The vein-stuff is quartz containing copper pyrites; the gold is contained in the copper pyrites, or disseminated in very minute grains through the quartz. Several of these veins are being worked; and their peculiarities were noticed by the author.

2. "Note on the Relations of the supposed Carboniferous Plants of Bear Island with the Palæozoic Flora of North America." By J. W. Dawson, LL.D., F.R.S., F.G.S.

The author referred to Dr. Heer's paper on the Carboniferous Flora of Bear Island (see Q. J. G. S. vol. xxviii. p. 161), and stated that the plants cited by Dr. Heer as characteristic of his "Ursa stage," are in part representatives of the American flora belonging to what the author has called the "Lower Carboniferous Coal-measures" (Subcarboniferous of Dana). He considered that the presence of Devonian forms was due either to the mixture of fossils from two distinct but contiguous beds, or to the fact that in these high northern latitudes there was an actual intermixture of the two

floras. He dissented altogether from Dr. Heer's identification of these plants with those of the Chemung group, or with those of the Middle Devonian of New Brunswick.

3. "Further Notes on Eocene Crustacea from Portsmouth." By Henry Woodward, Esq., F.G.S.

In this paper, after referring to his former communication on Crustacea from the Lower Eocene deposits at Portsmouth (Q. J. G. S. vol. xxviii. p. 90), the author gave a full description of *Rhachiosoma bispinosa*, one of the new species described in it, the materials being furnished by several fresh specimens, which show the whole structure of the animal. The new points include the description of the limbs, the anterior border of the carapace, the lower surface of the body in both sexes, and the maxillipeds.

The author also characterized, under the name of *Litoricola*, a new genus of Shore-crabs allied to *Grapsus*, from the same deposits. Of this genus he described two new species, *L. glabra* and *L. dentata*.

4. "On a new Trilobite from the Cape of Good Hope." By Henry Woodward, Esq., F.G.S.

The Trilobite described in this paper is from the Cock's-comb Mountains at the Cape of Good Hope, and was preserved in a nodule, the impression retained in which, when broken, furnished the most instructive details as to its structure. Each of the eleven thoracic segments was furnished with a long median dorsal spine, giving to the profile of the animal a crested appearance; on each side of this the axis of the segment bears two or three tubercles, and the ridge of the pleura four or five tubercles. The tail is terminated by a spine more than half an inch in length; and all the spines are annulated. For this Trilobite the author proposed the name of *Encrinurus crista-galli*, although with some doubt as to the genus, the head being only imperfectly preserved.

5. "On an extensive Landslip at Glenorchy, Tasmania." By S. H. Wintle, Esq.

In this paper the author described the effects of an extensive landslip from the northern face of Mount Wellington, in Glenorchy, about 5 miles from Hobart Town. It took place during the night of the 4th June, 1872, after a rainfall of $4\frac{1}{2}$ inches in twenty-four hours. The *débris* descended nearly 2000 feet, into the bed of the rivulet of Glenorchy. By the force of the accompanying torrent great quantities of huge trees, some of them 200 feet long, were piled up in vast heaps, mixed with boulders, agricultural implements, fences, and other objects. The trees were deprived of bark, branches, and roots. The Carboniferous limestone forming the bed of the rivulet was exposed by the washing of the torrent for more than two miles; natural sections showed the blue shelly limestone alternating with beds of mudstone and shales. At one part the author found both banks of the rivulet lined with small, sharply angular fragments of dioritic greenstone from the summit of the mountain;

large blocks of the same rock also occurred. The author described the beds displayed in a section close to the base of the great land-slip, above which is a smooth surface of greenstone, covered with prostrate trees and immense blocks of greenstone half buried in yellow clay and sludge. The whole neighbourhood was described by the author as presenting evidences of former landslips. The author further described the appearances presented by the upper winding part of the gully traversed by the torrent, and, in conclusion, noticed certain results of similar phenomena as displayed in the same district. The paper was illustrated by three stereoscopic views.

LI. *Intelligence and Miscellaneous Articles.*

A NEW DETERMINATION OF THE VELOCITY OF LIGHT.
BY M. A. CORNU.

I HAVE the honour to present to the Academy the definitive result of my researches relative to the determination of the velocity of light, undertaken three years since. In a preceding communication I have succinctly described the method of observation, which, in principle, is that of the toothed wheel, due, as well as the improvements which have been made in it, to M. Fizeau. Of the latter I will mention the electrical registry of the velocity of the mechanism —which it is necessary to ascertain at each instant in absolute value, because it is with it that the velocity of light is directly compared. By the regularity and concordance of the results of my first essays I was led to hope that, by repeating the experiment between two new stations at four times the distance from each other, I should be able to obtain a determination sufficiently precise to decide between the two values given, the one by the old astronomical data (308000 to 310000 kilometres in a second), the other (298000) by the experiments of Foucault with the rotating mirror. I am happy to announce that the accuracy I expected (" probably to less than a hundredth ") has been even exceeded; and the question of the absolute value of the velocity of light appears to me to be decided in favour of the lower number, as will be seen from the numerical results which I have obtained.

I will first briefly describe the arrangements. The observing-station is in an upper room of the pavilion of the École Polytechnique; the other, in the chamber of one of the barracks on Mont Valérien. At the former station are fixed the observing-telescope (aperture 180 millims., focal distance 2·4 metres), the toothed wheel and its motive mechanism, the illuminating system, the apparatus for registering the velocities, the electric wires, &c. The opposite station contains only the reflecting collimator, composed of an objective (aperture 110 millims., focal distance 1·2 metre) mounted as a telescope and with a small plane mirror of silvered glass in the focal plane.

I describe in detail in the memoir the precautions to be taken in order to adjust in the same right line the optic axes of the two apparatus, and to place the surface of the mirror in the focal plane of the collimator. This second condition must be fulfilled very accurately; otherwise the loss of light on the return would be considerable. I succeeded in fulfilling it completely by using as a reticule the silver pellicle partially raised on certain points of the mirror: the precision is then determined entirely by the defining-power of the telescope.

Among the improvements introduced in the course of these researches, I will mention the construction of the motor of the toothed wheel. Froment's motor, with helicoidal tooth-range, was given up, as it necessitated too great a motive force. I notably simplified the arrangement by utilizing some clockwork mechanisms sold under the name of *roulants carrés* (the sides from 12 to 15 centims.); the escapement and wheelwork are taken away, and the ratchet-wheel of the escapement is replaced by a lighter wheel with finer teeth: I used for this purpose three patterns, wheels with 104, 116, and 140 teeth.

By fitting a powerful spring in the barrel, I have been able to attain velocities of from 700 to 800 turns in a second. Finally, as a complement, on the axle of the minute-hand I disposed an electric cam (necessary for the registration of the velocity of rotation of the mechanism), a brake (to regulate the velocity at will), and a second barrel (permitting the toothed wheel to be turned in the contrary direction). This last arrangement is useful for the elimination of certain systematic errors which might result from the mechanism itself.

I shall not dwell upon the description of an experiment. The observer, attentive to the variations of intensity of the return-light, transmits electric signals to the recorder—that is, a cylinder covered with blackened paper, on which three electromagnets cause to be traced respectively the signals from the seconds-clock, from the cam of the mechanism, and from the key managed by the observer. In general he notes the successive disappearances of the light, which correspond to velocities of the toothed wheel, varying as the series of the odd numbers. Thanks to the working of the brake, he can at will produce an acceleration or retardation of the motion of the mechanism, or maintain a velocity sensibly constant during some seconds.

Notwithstanding the disadvantages of the atmosphere of Paris, I often obtained a very intense return-light with the oxyhydrogen lamp, or even with a simple petroleum-lamp. The total number of my observations exceeds a thousand; they are registered in the form of graphic traces, which I have the honour to lay before the Academy. The work of reduction is rather tedious; so I have taken up only those observations which are most complete and were made under satisfactory circumstances; their number amounts to about 650. A uniform method of calculation permitted me to deduce from the traces the time which the light occupied in accom-

plishing the double of the distance between the two stations. This distance, carefully determined, was found to be 10310 metres, with a probable error of less than 10 metres in excess or defect—that is to say, an approximation to within one thousandth part. I effected this measurement myself, with the aid of a little triangulation. I took advantage of the circumstance that the three salients of bastions No. 1, No. 2, and No. 5 of the fortifications of Mont Valérien could be seen from the belvedere of the École Polytechnique. Taking, with a good azimuth-circle, the angles subtended by those three points, I was able, by the calculation of the proper segments, to obtain the data necessary for ascertaining the distance between the two stations. The dimensions of the fortress having been accurately measured by the engineers, I obtained from the Dépôt des Fortifications the distances between the necessary points, which enabled me to dispense with measuring a base.

I found, besides, at the Prefecture of the Seine, two determinations of the distance from certain points on Mont Valérien to the Panthéon (one derived from the operations for the Register, the other from those of the Commission for the Plan of Paris). The mean of the three closely agreeing values thus obtained gave the number above adopted. If a subsequent geodesic operation should furnish more exactly the distance between the two stations, it will be very easy to calculate the correction to be applied to my results.

The following Table gives the result of the definitive calculations; the values of the velocity of light, expressed in kilometres per second, are classed according to the order of the occultations of the return-light which furnished them.

Order 1.	Order 2.	Order 3.	Order 4.	Order 5.	Order 6.	Order 7.
—	302600	297300	298500	298800	297500	300400
—	(17)	(236)	(376)	(480)	(91)	(27)

The numbers in parentheses express the relative *weight* of the corresponding values. They were formed by dividing by 10 the product of the number of observations into $2n-1$ (n being the order of the occultation) and the coefficient 1, 2, 3, or 4, according as the remark in the experiment-book was "pretty good," "good," "very good," or "excellent," according to the state of the atmosphere.

The compound mean gives 298400. Multiplying this by the index of refraction of the air, 1·0003, we obtain the number 298500 kilometres per second as the value of the velocity of light *in vacuo* deduced from the totality of my observations. I reckon that this number approaches within $\frac{1}{300}$.

The accordance of this result with Foucault's is not uninteresting; and it should be remarked that Foucault's experiments required verification, not only because the details of the observations and procedure have not been published, and therefore have undergone no discussion, but also because the rotating-mirror method is exposed to grave objections (into the statement of which I shall not now enter), while M. Fizeau's method is free from those objections.

Astronomers, also, will find in this new determination of the velocity of light an important confirmation of the value of the sun's parallax 8″·86, which is obtained by comparing this number with the constant of the aberration. It is the value which M. Le Verrier has found again by three series of observations relative to the motions of the planets, particularly Mars and Venus. The importance, then, for astronomy, of the precise determination of the velocity of light cannot be too much insisted on.

In conclusion, I think I am right in affirming, as M. Fizeau announced immediately after his first researches, that the same experiments could, without much more difficulty, be repeated, under favourable atmospheric and topographical conditions, with stations from 20 to 30 kilometres apart. Then, with the aid of a special geodesic operation, I doubt not that we could obtain a determination of the velocity of light approximate within less than a thousandth part. I have the greatest desire to attempt this experiment, and should deem it a great honour if the Academy would receive the project favourably. It is desirable, for the honour of French science, that those grand labours commenced by Rœmer at the Observatory of Paris, simplified and continued by French savants, should be completed in France with all the precision which comports with their importance in the light of physics and astronomy. —*Comptes Rendus de l'Académie des Sciences*, vol. lxxvi. pp. 338–342.

NEW EXPERIMENTS ON SINGING FLAMES. BY FR. KASTNER.

If two flames of suitable size be introduced into a glass tube and both placed at two thirds of the length of the tube from its lower end, they will vibrate in unison. The production of the phenomenon continues as long as the flames are kept separate; but the sound ceases as soon as the two are put in contact.

I took a glass tube 55 centims. in length, 41 millims. in exterior diameter, and 2·5 millims. in thickness. Two separate flames, produced by the combustion of hydrogen gas issuing from burners of suitable construction, placed at 183 millims. from the base gave the sound of FA natural.

As soon as, with the aid of very simple mechanism, the flames are brought together, the sound is suddenly interrupted. If we vary the position of the flames in the tube, leaving them still separate, above the third part of the length the sound diminishes as far as the middle of the tube, beyond which all sound ceases; below the same point, on the contrary, the sound augments as far as the quarter length of the tube. If at this place the flames are brought together, the sound does not cease immediately, as the two flames can continue to vibrate as a single one.

The interference of singing flames is only produced under special conditions. It is of importance that the length of the tubes be in accordance with the number of the flames; the height of the flame exerts only a limited action upon the phenomenon, while the form of the burners plays an important part.

The whole of the experiments which I have made during the last two years have led me, as an application, to the construction of a musical instrument of an entirely new quality of tone, approximating to the human voice. I have given it the name of *pyrophone*. This instrument is composed of three key-boards, which can be united like those of the organ. With the aid of very simple mechanism, all the keys are connected with the feed-pipes of the flames in the glass tubes. When one of the keys is pressed, the flames separate and the sound is immediately produced; as soon as the performer ceases to act on the key, the flames come together and the sound ceases.—*Comptes Rendus de l'Académie des Sciences*, March 17, 1873, p. 699.

ON A NEW OPERATION BY WHICH THE VELOCITY OF PROJECTILES CAN BE DETERMINED OPTICALLY. BY MARCEL DEPREZ.

An important advantage in using artillery would be an exact knowledge of the form of the trajectory of projectiles fired under high angles, as well as their velocity at each point of the trajectory. Unfortunately the methods hitherto applied to very flat trajectories are totally inapplicable to firing under high angles. In reflecting on the means for supplying this deficiency, I have been led to devise a process which, applicable to many other questions besides that dealt with in balistics, I think it will be useful to make known.

Suppose that on the ground-plan of the polygon two stations, A and B, be chosen, at each of which is placed a telescope. The optical axes of these telescopes must be in one and the same vertical plane perpendicular to the vertical plane taken by the axis of the piece; and the stations A and B are to be situated at nearly equal distances from the intersection of the two planes. The projectile is to be furnished with a fusee shedding a bright light (magnesium would doubtless be very suitable for the purpose). This being admitted, we shall suppose that, the piece being pointed at a constant angle and firing several times in succession with the same charge and the same projectile, the resulting trajectories will pass constantly within the field of the telescopes. Then it is evident that, knowing the angles α and β which the optic axes of the latter make with the *base* A B at the moment when the two observers simultaneously perceive the projectile, as well as the length of that base, we shall have all the elements necessary for determining the coordinates of the intersection of the trajectory and the vertical plane passing through A B. This process, however, in principle the same as that employed for bolides, would not give the velocity of the projectile at the moment it passes before the observers. To determine this element I propose the following means :—

With each of the telescopes is coupled a parallel telescope, of the same power, furnished with a reticule the wires of which subtend

known angles ; but the objective, instead of being fixed, is mounted upon one of the branches of a diapason animated, during the experiment, with a known vibratory movement. In this second telescope the eyepiece is replaced by a plane mirror inclined 45° to its optic axis, and reflecting the luminous rays to a parallel mirror situated in the first telescope. This second mirror is transparent, so that it transmits the rays coming from the fixed objective. The observer, then, perceives simultaneously two luminous images of the trajectory. That given by the fixed telescope will appear under the form of a rectilinear stroke of fire ; the other, transmitted by the vibrating objective, will have the form of a sinusoid, of which the rectilinear stroke will be the axis. If the number of vibrations of the diapason in the unit of time be chosen so that the number of branches of the sinusoid comprised between two parallel wires of the reticule does not exceed five or six, the observer will be able to count them instantaneously, and also to remember which wires pass through the intersection of the sinusoid with its central line. These two elements will give immediately the *angular* velocity of the projectile ; and on multiplying it by the distance from the observer to the projectile (given by the operation previously described), we shall have the *linear* velocity sought.

The following is a second process, based on the same principle. Its realization will, it is true, be more complicated ; but it will have the double advantage of offering several means of controlling the results obtained and of occasioning less fatigue to the observer. Let the vibrating lens be replaced by a wheel carrying, say, 5 lenses at its periphery, and making 20 turns in a second. At the instant of the passage of the projectile the observer will see parallel rectilinear strokes of fire, the number of which will be proportional to the time employed by the projectile in traversing the field of the telescope. It will then be sufficient for him, as before, to remember the number of strokes comprised between two wires, in order thence to infer the velocity sought. In fact, each of the bright strokes sent by the moving lenses represents in direction the resultant of a parallelogram, the sides of which are proportional to the *angular* velocities of the projectile and of the lenses (round the observer as a centre). Now the magnitude and direction of the velocity of the lenses are known ; and, knowing the directions of the resultant velocity and of the velocity of the projectile, we shall be able to find the amount of the latter. To determine with precision the directions of these two velocities, it will only be requisite to put one of two special wires of the reticule parallel to each of them ; and this operation (provided the successive discharges of the gun be identical) will be susceptible of greater precision than that described in the first process.

These means are evidently applicable to all bodies in motion ; and it is precisely by making use of them to measure velocities known beforehand that we shall be able to judge of the degree of precision to be attained by them. Not being in favourable circumstances for carrying out experiments of this kind, I should be

happy if this communication should determine others to try them. The limits of this note compel me, also, to pass over in silence many details intended to augment the chances of success.—*Comptes Rendus de l'Acad. des Sciences*, vol. lxxvi. pp. 819–821.

ON THE DEVELOPMENT OF HEAT BY THE FRICTION OF LIQUIDS AGAINST SOLIDS. BY O. MASCHKE.

It is known that the energetic aspiration of a liquid through a porous substance is accompanied by a rise of temperature, probably resulting from the friction of the liquid against the walls of the capillary channels into which it rushes. M. Maschke gives some measurements of the rise of temperature obtained by soaking amorphous silica with various liquids.

The porous body, reduced to grains of the size of mustard-seed, is introduced into a test-tube closed by a stopper with three apertures, one of which gives passage to a thermometer, which measures the temperature of the porous body and afterwards that of its mixture with the liquid introduced; another has a glass siphon which dips to the bottom of the test-tube; through the third an aspiration-tube passes. By a prolonged stay in the same medium, the porous body and the liquid are caused to have, at the commencement of the experiment, sensibly the same temperature. The outer extremity of the siphon being immersed in the liquid, the operator inhales gently through the discharge-tube, and observes the highest temperature reached by the thermometer during the imbibing of the liquid by the porous substance.

M. Maschke has studied the following cases:—amorphous silica, first moistened, then dried at a moderate temperature till it contained only 39·8 per cent. of water, treated with water; silica containing 18·8 per cent. of water, and water; dry silica and water; calcined silica exposed to moist air (22·68 per cent. H^2 O), and water; silica calcined and then exposed to very moist air (28·24 per cent. H^2 O), and water; silica calcined and cooled over sulphuric acid, treated sometimes with water, and sometimes with benzole, oil of almonds, concentrated sulphuric acid, or alcohol.

The experiment lasted 3, 10, 20, 30, and even as long as 45 minutes; but it was not at the end of the experiment that the thermometer immersed in the porous substance attained its maximum.

The author operated at a mean temperature in the vicinity of 15° or 20° C. In the majority of instances the rise of temperature varied between 1° and 8° C. In the calcined and dry silica soaked with concentrated sulphuric acid the thermometer rose from 19°·8 to 33°·5. In one part of calcined silica mixed with 3·2 parts of alcohol the thermometer rose from 13° to 26° C. Quartz and pounded glass treated in the same manner give no appreciable rise of temperature.—*Archives des Sciences Physiques et Naturelles*, vol. xlvi. p. 271.

LII. *On the Expansion of Superheated Vapours.*
By Dr. Hermann Herwig[*].

§ 1.

IN my earlier investigations on the variability of vapour-den-
sities[†], among other results it appeared that, under certain
feeble pressures, continually heated vapours contained within a
space of constant dimensions may have a smaller coefficient of
expansion than that which is attributed to perfect gases, viz.
0·003663. The numbers then obtained, however, rather pointed
towards such a behaviour of the vapours than decidedly proved
it, as the differences in this direction of the observed densities
amounted to very small values[‡]. On this account I have en-
deavoured by another method to elucidate further this still dis-
putable point.

The procedure consists in comparing directly the expansion
under pressure of a constant volume of vapour with that of a
constant volume of dry air. Superheated vapour and dry air
were contained in the two legs of a U-shaped glass tube herme-
tically sealed at both ends, and were separated from each other
by an extensive thickness of mercury. The preparation of such
tubes, in which vapour and air have any pressure we please, is
very simple. A wide tube, as nearly as possible cylindrical, is
bent into a U-shape; its two ends are drawn out thin; and it
is filled to fully one half with strongly heated perfectly dry mer-
cury. Then a quantity of liquid, the vapour of which is to be
investigated, is drawn into one of the legs; and now the tube,
with the exception of its capillary ends, is put into a heated

* Translated from a separate impression, communicated by the Author,
from Poggendorff's *Annalen*, vol. cxlvii. p. 161.
† Pogg. *Ann.* vol. cxxxvii. pp. 19 & 592, and vol. cxli. p. 83.
‡ Compare the remarks, Pogg. *Ann.* vol. cxli. p. 89.

water-bath, in order by violent boiling of the liquid to expel the
air from the leg of the tube and fill it exclusively with a certain
quantity of the vapour.   When the wished-for degree of filling
is attained, this leg is closed by melting the end with a pointed
flame.   As the boiling takes place under atmospheric pressure,
when we wish the half-leg filled with superheated vapour of
feeble pressure we have to keep only a corresponding fraction of
the interior of the leg free from mercury during the boiling, conse-
quently to put it in an inclined position, and to close the end as
soon as the small space left free is filled with pure saturated vapour
under atmospheric pressure. If, on the contrary, a greater pres-
sure of vapour is required, a larger space is to be taken for the
vapour during the boiling; consequently the tube must be in-
clined in the opposite direction ; moreover the tube can be closed
before the liquid is vaporized.   In every case it is easy to secure
the filling of the leg with the exact quantity desired of the vapour
of the liquid in question, and free from air; of this we may
convince ourselves by letting the tube cool, when the vapour
becomes liquid again, and the entire half of the tube must be
occupied exclusively by mercury and a small quantity of the
liquid.   This was always perfectly accomplished in my expe-
riments.

The other leg is then, after the intervention of a drying-appa-
ratus, attached to the air-pump ; and when, after several ex-
haustions, it has been filled with dry air of the requisite density,
it also is hermetically sealed.

We have thus a completely closed tube which, starting from
a certain temperature, contains at all higher temperatures super-
heated vapour on one side, and dry air on the other, of each a
fixed quantity.   Both vapour and air then exhibit their pressure-
difference for each temperature through the difference of level of
the mercury between them in the lower half of the tube, and
therewith occupy each a determinate space.   To ascertain with
exactness these spaces, I drew some strokes with a diamond upon
the tube, and then placed it, supported on a strong brass foot,
in a large water-bath, the fore and hind sides of which were
formed by plane-parallel looking-glass plates.   The bath stood
upon a table fixed to the wall, in front of a window of the labo-
ratory, and was heated by gas-flames.   A uniform temperature
in the bath was secured by means of a stirrer with a double
frame, which was moved up and down from 20 to 30 times in a
minute by a small steam-engine.

The observations were conducted as follows.   I first produced
in the bath the highest temperature that was to be attained by
the tube during the experiments, in order to make sure of ex-
pelling from the upper layers of the mercury any particles of

the vapour of the liquid or of air which they might still retain. In this way the subsequent observations were certainly made upon constant quantities of vapour and of air (so far as the adhesion-phenomena to be afterwards considered did not come into play), of which I in many cases convinced myself by repeated series of observations with falling and with rising temperature.

I now made the observations for a series of temperatures, each of which was kept constant for a longer time by brisk action of the stirrer; and to read off the difference of level of the mercury for each temperature a delicate cathetometer was employed. The cathetometer (an excellent specimen made by M. Schubart, of Ghent) stood upon a massive stone pillar which is erected directly above the subterranean vault of our laboratory; the slightest shaking of the cathetometer is thus excluded. The temperature of the bath was taken by a delicate Geissler's normal thermometer which gives tenths of a degree. The subsequent calculation shows, moreover, that a reading of the temperature to tenths of a degree would really have been unnecessary—that, in such observations of differences, it is much more essential to have the same temperature in all parts of the tube; and this, as I have said, was always attained by the perfect effectiveness of the stirring-apparatus.

It was then only necessary, besides the observations of the level of the mercury, to note down at the same time with each observation its position about the strokes which were marked on the tube.

After carrying out these observations, I broke off the capillary end of the tube on the vapour side, and thus put this portion of the tube in communication with the external air. The pressure p_0 of the dry air in the other leg of the tube, at a fixed temperature θ of the bath, was then again determined, and the position of the mercury about the diamond-marks noted down.

Lastly the following weighings of the mercury were made:—

    (1) The weight of the mercury contained in the tube during the experiments; let it be W.

    (2) The weight of the mercury which, at a certain temperature θ fills the whole tube; let it be x.

    (3) The weight of the mercury which at the temperature θ_1 fills the space occupied by the vapour during the measurement when the position of the mercury about the diamond-marks was noted down; let it be y.

    (4) The weight of the mercury which at the temperature θ_2 fills the space occupied by the dry air at the time of the determination of its pressure in comparison with that of the external air; let it be z.

(5) The weight of the mercury which occupies the length of a millimetre in the cylindrical part of the tube at the temperature θ_3; let this be u.

With these data the entire calculation can be effected.

§ 2.

Two formulæ especially present themselves from which we may endeavour to judge of the behaviour of superheated vapour. Let p, v, and t be the values of pressure, volume, and temperature (Celsius) of a determined weight of superheated vapour; we can either put

$$pv = \phi(273 + t),$$

where ϕ is then a variable whose law of variation must be sought, or we can put

$$pv = C(E + t),$$

where E is such a variable, but C is a constant dependent on the quantity by weight of the vapour.

The latter formula was applied by Fairbairn and Tate[*] to their observations on steam (to be considered further on). I have not chosen it, because, with the proportions of my experiments, the results calculated after this formula must have given less striking numbers than those calculated according to the first. We shall easily be convinced of this from the following.

Thus, if we take the formula

$$pv = \phi(273 + t),$$

it is only when the vapour exhibits the behaviour of a perfect gas that ϕ becomes a constant depending on the weight of the vapour. At the same temperature t, for dry air, we have

$$PV = R(273 + t),$$

where P and V denote pressure and volume, and R is a constant depending on the quantity of air employed. This equation presupposes, of course, the full validity of Mariotte and Gay-Lussac's law.

From the two equations we get

$$p - P = (273 + t)\left(\frac{\phi}{v} - \frac{R}{V}\right).$$

Putting $p - P = H,$

we get

$$\phi = v\left(\frac{H}{273 + t} + \frac{R}{V}\right),$$

according to which I have calculated my observations.

* Phil. Trans. 1862, p. 591.

H is the difference (reduced to 0°) of the levels of the mercury in the tube at each experiment, and so the proper object of the measurements. In the calculations, H is given in millimetres.

v, R, and V are obtained in the following manner from the mercury-weighings:—

If we imagine the tube divided into two exactly equal parts, and if W_2 is the volume of each part at 0° expressed in the weight of mercury at 0°, at the temperature t of observation it becomes $W_2(1 + \beta t)$, if β is the coefficient of expansion of the glass. If, now, during the observation made for the temperature t the weight of the mercury in the vapour-half of the tube is found to be W_1, this occupies the space $W_1(1 + \gamma t)$, where γ is the known coefficient of expansion of mercury at that temperature. The vapour-volume v at the temperature t is evidently equal to the difference,

$$v = W_2(1 + \beta t) - W_1(1 + \gamma t).$$

But, from the mercury-weighings mentioned in the preceding section, we obtain

$$W_2 = \frac{x}{2} \cdot \frac{1 + \gamma \theta}{1 + \beta \theta}.$$

$W_1(1 + \gamma t) = W_2(1 + \beta t) - y[1 + \gamma \theta_1][1 + \beta(t - \theta_1)]$, if for the temperature t we first select exactly that temperature of observation at which we at the same time noted down the position of the mercury about the diamond-strokes.

Just so

$$V = W_2(1 + \beta t) - (W - W_1)(1 + \gamma t);$$

and, finally,

$$R = \frac{\alpha p_0 z[1 + \gamma \theta_2][1 + \beta(\theta - \theta_2)]}{1 + \alpha \theta},$$

where α is the well-known value 0·003663.

It will at the same time be seen that all the space-determinations are effected with the same unit, mercury at 0°.

For any other t, v and V acquire other values only so far as W_1 changes with t; while W_2 and W remain constant for all values. On account, however, of the cylindricity of the part of the tube which here comes into consideration, we must put

$$\frac{dW_1}{dt} = - \text{const.} \frac{dH}{dt};$$

that is, according to No. 5 of the above mercury-weighings, remembering that H is given in height of mercury at 0°,

$$\frac{dW_1}{dt} = -u(1 + \gamma \theta_3) \frac{dH}{dt} = -c \frac{dH}{dt}.$$

Therefore, W_2, W, R, and c once calculated for a series of observations, the rest of the calculation is performed with the two formulæ

$$\phi = [W_2(1+\beta t) - W_1(1+\gamma t)]$$
$$\left\{ \frac{H}{273+t} + \frac{R}{W_2(1+\beta t) - (W-W_1)(1+\gamma t)} \right\}$$

and

$$\frac{dW_1}{dt} = -c\frac{dH}{dt}.$$

It will now be seen from the following that the first term in the right-hand brackets for ϕ, on account of the small value of H in the greatest number of my series of observations, is decidedly the smaller of the two bracketed terms. An at all events small error in the determination of t would, on this account, have absolutely no influence; hence this formula was especially advantageous for the calculation of the experiments.

§ 3.

Before communicating the observations on vapours, I will first cite two tests of accuracy to which I subjected the method. First, I filled a tube on both sides with dry air; constant values of ϕ must evidently then be obtained. The two following Tables contain the results from two tubes. On their arrangement nothing more need be added after what has been said.

TABLE I.

$W_2 = 307.6$. $W = 224.5$. $R = 414.674$. $c = 0.88$.

t.	H.	W_1.	v.	V.	ϕ.	p.
	millims.					millims.
30.7	+46.35	89.59	217.75	172.1	557.90	778
42.5	46.45	89.5	217.7	171.9	557.21	
52.3	46.45	89.5	217.65	171.7	556.73	
65	46.65	89.31	217.7	171.3	557.04	865

TABLE II.

$W_2 = 309.6$. $W = 224.5$. $R = 387.472$. $c = 0.88$.

16.3	55.8	81.24	228.25	166.05	576.64	731
28.2	56.1	80.98	228.4	165.55	577.11	
42.4	56.3	80.8	228.5	165.1	577.05	
58.7	56.4	80.71	228.4	164.7	576.17	837

The constancy of ϕ is sufficiently attained; it is seen, however, within what limits small variations occur.

In the second the question was, to realize a case in which a variable ϕ might be surely expected. I endeavoured to make use of my earlier experience *, according to which, at not high temperatures small quantities of aqueous vapour exhibit extraordinary power of adhesion to glass and mercury. I put into a tube a portion of dry mercury, and filled up one side with dry air; into the other side I then poured a quantity of somewhat moist mercury: the rest of this side was likewise filled with air and hermetically sealed. I now heated it strongly, in order to expel the water from the mercury and so fill the air above it with aqueous vapour. A sufficient quantity of vapour actually entered the space which the air occupied, as the following series of measurements show; they contain the corresponding values of t and H.

TABLE III.

t.	H.	t.	H.	t.	H.
°	millims.	°	millims.	°	millims.
41·6	−113·5	18·3	−104·8	64·9	−107·8
51·7	−115·4	14	−105·3	64·7	−107·3
61·9	−116·9	21	−104·5	70·5	−107·7
71·5	−116·4	25·1	−104	70·7	−107·8
78·3	−112·7	30·1	−103·2	76·1	−108·7
83·7	−111·3	35	−103·5	76	−109
90	−111·2	40·3	−104	75	−108·7
93	−111	46·2	−104·8	80·5	−109·5
85	−110·2	32·7	−103		
78·3	−109·6	50·4	−105·5		
13·6	−106·3	60·1	−106·9		
26·1	−103·9	70·5	−108·4		
34·8	−103·3	80·4	−109·9		
48·2	−105·1	89·8	−110·9		
58·1	−106·7	94·4	−111		
48·2	−105·1	83·6	−109·8		
41	−103·9	73·6	−108·4		
29·8	−102·7	64·1	−107·1		
26·5	−103·5	54·9	−105·4		
		54	−105·2		

This Table shows that as far as 90°, during the first heating the air on the side of the moist mercury exerted a considerably less pressure than afterwards, when the steam expelled by the heating cooperated.

In the numbers which were obtained after this high temperature had once been reached, such an agreement is recognized in the repeated ascending and descending series, that greater could not be expected if adhesion of the aqueous vapour took place.

* Pogg. *Ann.* vol. cxxxvii. p. 602.

These numbers are arranged in the following Table according to the temperatures, and, with their mean values (given under the heading "H corrected"), made use of for the calculation of the values of ϕ. In another column the variation of ϕ for each $10°$ of change of temperature is given in percentages of ϕ—thus, $\dfrac{100}{\phi} \cdot \dfrac{10\Delta\phi}{\Delta t}$.

TABLE IV.

$W_2 = 390$. $W = 400$. $R = 144$. $c = 0.722$.

t.	H.	H corrected.	W_1.	v.	V.	ϕ.	$\dfrac{100}{\phi} \cdot \dfrac{10\Delta\phi}{\Delta t}$.	p.
°	millims.							millims.
13·6	−106·3	−105·7	245·73					
14	−105·3	−125·7	245·73	143·9	235·5	34·97	} 11	70
18·3	−104·8	......	245·08	144·3	234·7	36·62	
21	−104·5	......	244·86	144·4	234·45	37·36	} 9
25·1	−104	......	244·5	......	......	......	} 8
26·1	−103·9	−103·7	244·28	......	....:.	......	
26·5	−103·5	−103·7	244·28	144·8	233·8	39·04	
29·8	−102·7	−102·9	243·71	......	......	......	} 9
30·1	−103·2	−102·9	243·71	145·25	233·15	40·40	
32·7	−103	−103·1	243·85	......	......	......	} 1·5
34·8	−103·3	−103·4	244·07	......	......	......	
35	−103·5	−103·4	244·07	144·7	233·4	40·70	} 1·2
40·3	−104	......	244·5	144·1	233·7	40·96	
41	−103·9	−104	244·5	......	......	......	
46·2	−104·8	......	245·08	......	......	......	
48·2	−105·1	......	245·29	......	......	......	} 0·8
48·2	−105·1	......	245·29	......	......	......	
50·4	−105·5	−105·2	245·37	142·9	234·4	41·30	
54	−105·2	−105·3	245·44				
54·9	−105·4	−105·3	245·44				
58·1	−106·7	......	246·45				
60·1	−106·9	......	246·59	141·5	235·5	41·08	
64·1	−107·1	......	246·74				
64·7	−107·3	−107·6	247·1				
64·9	−107·8	−107·6	247·1				
70·5	−107·8	−108	247·39				
70·5	−108·4	−108	247·39	140·1	236·1	41·40	
70·7	−107·7	−108	247·39				
73·6	−108·4	......	247·68				
75	−108·7	......	247·89				
76	−109	−108·9	248·04				
76·1	−108·7	−108·9	248·04				
78·3	−109·6	......	248·54				
80·4	−109·9	−109·7	248·62				
80·5	−109·5	−109·7	248·62	138·5	237·15	41·19	
83·6	−109·8	......	248·69				
85	−110·2	......	248·98				
89·8	−110·9	......	249·48				
90	−111·2	−111	249·55				
93	−111	......	249·55				
94·4	−111	......	249·55	137·05	237·85	41·57		111

The values of ϕ therefore hold good for air containing a cer-

tain quantity of aqueous vapour, which at low temperatures partially adheres and in various proportions at various temperatures. It is seen what great changes this adhesion, consequently the variable going out of play of a certain quantity of vapour, occasions in the values of ϕ belonging to low temperatures.

The method has accordingly been proved by a twofold test to be perfectly applicable; and this is the direct confirmation of an antecedently justified expectation.

§ 4.

The vapours which I investigated by this method were sulphide of carbon, chloroform, and ethylic alcohol. The chloroform was a small, carefully preserved remainder of the preparation made use of in my earlier experiments; the two others were newly prepared. In the following the Tables on the vapour of sulphide of carbon shall first be given. In the last column the vapour-pressures p are still shown; they are calculated from ϕ by the formula

$$p = \frac{\phi(273+t)}{v}.$$

It may further be remarked that the *absolute* values of ϕ and p are affected by at any rate small errors in the above-mentioned weighings of the individual volumes of the tubes, which cannot be excluded; and therefore in this point the possibility of small errors must be borne in mind. The *course* of the values of ϕ, however, which is alone of importance, depends essentially upon very exactly observed values of H. Where several series of observations gave somewhat differing numbers for the latter values, I have noted down, in the column " H corrected," the numbers made use of for the calculation; I have also inserted in this column notices of the cessation of saturation. Here, however, according to the Tables, the difference is never more than 0·1 millim.

Sulphide of Carbon.

Table V.

$W_2 = 378\cdot4.$ $W = 408.$ $R = 67\cdot425.$ $c = 0\cdot824.$

$t.$	H.	H corrected.	$W_1.$	Vapour-volume. $v.$	Air-volume. $V.$	$\phi.$	Vapour-pressure. $p.$
	millims.						millims.
$27\cdot2$	$+5\cdot75$	......	$202\cdot9$	$174\cdot75$	$172\cdot5$	$71\cdot65$	123
$41\cdot2$	$5\cdot85$	......	$202\cdot82$	$174\cdot45$	$172\cdot1$	$71\cdot59$	129
$55\cdot8$	$5\cdot95$	......	$202\cdot74$	$174\cdot1$	$171\cdot6$	$71\cdot56$	135
62	6	......	$202\cdot69$	174	$171\cdot4$	$71\cdot56$	138
$71\cdot5$	$6\cdot1$	......	$202\cdot61$				
72	$6\cdot1$	......	$202\cdot61$	$173\cdot8$	171	$71\cdot60$	142
$74\cdot5$	6	$6\cdot1$	$202\cdot61$				
$88\cdot2$	$6\cdot2$	......	$202\cdot53$	$173\cdot4$	$170\cdot4$	$71\cdot59$	149

TABLE VI.

W$_2$=503·8.   W=572.   R=124·956.   c=1·087.

t.	H.	H corrected.	W$_1$.	Vapour-volume. v.	Air-volume. V.	φ.	Vapour-pressure. p.
°	millims.						millims.
11·2	−31·1	......	283·1	220·3	214·45	104·26	134
18	−31·3	......	283·32	219·8	214·4	104·39	138
25·4	−31·7	......	283·75	219·05	214·55	104·31	142
26·6	−31·7	......	283·75	219	214·5	104·41	143
34·3	−31·9	......	283·97	218·5	214·4	104·66	147
38·2	−32·1	......	284·19	218·1	214·5	104·56	149
44·8	−32·35	......	284·46	217·6	214·5	104·61	153
55	−32·6	......	284·73	216·9	214·35	104·88	159
57·2	−32·7	......	284·84	216·7	214·4	104·83	160
59·2	−32·7	......	284·84	216·6	214·3	104·98	161
64·9	−33	......	285·16	216·2	214·4	104·89	164
80·4	−33·2	......	285·38	215·2	213·8	105·56	173
89	−33·6	......	285·82	214·45	214·1	105·25	178

TABLE VII.

W$_2$=1110.   W=1251.   R=230·241.   c=2·408.

t.	H.	H corrected.	W$_1$.	Vapour-volume. v.	Air-volume. V.	φ.	Vapour-pressure. p.
12·6	+7·3	......	593·52	515·5	451·4	276·11	153
18	7·4	......	593·28	515·3	450·6	276·40	156
19·8	7·4	......	593·28				
25·8	7·55	7·5	593·04				
30·2	7·45	7·5	593·04	514·5	449·25	276·40	163
32·2	7·55	7·5	593·04				
42·1	7·55	......	592·92				
53·5	7·7	......	592·56				
56·3	7·6	7·7	592·56	512·9	446·4	276·53	178
57·5	7·7	......	592·56				
74·6	7·8	......	592·32				
76·3	7·9	7·8	592·32	511·6	444·3	276·54	189
87·6	7·85	......	592·2				
94·7	7·85	......	592·2	510·3	442·4	276·47	199

TABLE VIII.

W$_2$=1023·7.   W=1160.   R=209·6.   c=2·76.

t.	H.	H corrected.	W$_1$.	Vapour-volume. v.	Air-volume. V.	φ.	Vapour-pressure. p.
9·3	+15·2	......	529·6	493·45	392·5	290·07	166
19·7	15·25	......	529·46	492·85	391·4	289·61	172
24·6	15·3	......	529·32				
28·7	15·3	......	529·32	492·35	390·5	289·23	177
34	15·3	......	529·32				
35·5	15·4	......	529·05				
43·3	15·5	......	528·77	491·9	388·6	289·42	186
50·1	15·65	15·6	528·5				
53·6	15·55	15·6	528·5	491·4	387·4	289·34	192
64·2	15·8	......	527·91				
65·2	15·8	......	527·94	491·2	385·8	289·81	199
79·3	15·95	......	527·53	490·6	384·1	289·83	208

TABLE IX.

$W_2 = 386.4.$ $W = 404.$ $R = 146.78.$ $c = 0.743.$

t.	H.	H cor-rected.	W_1.	Vapour-volume. v.	Air-volume. V.	ϕ.	Vapour-pressure. p.
°	millims.						millims.
16·6	− 2·4	......	231·23	154·6	213·3	105·10	197
28·3	− 2·4	......	231·23	154·25	213	105·14	205
39·7	− 2·3	......	231·15				
49·2	− 2·2	......	231·08	153·7	212·4	105·16	220
57	− 2·2	......	231·08				
65·8	− 2·1	......	231·01				
76·5	− 2	......	230·86	153	211·6	105·25	240

TABLE X.

$W_2 = 525.4.$ $W = 576.$ $R = 134.187.$ $c = 1.02.$

17·6	+ 7·3	7·26	250·9	273·9	199·5	191·07	203
25·2	7·2	7·26	250·9	273·7	199·15	191·07	208
25·6	7·3	7·26	250·9				
32	7·25	7·26	250·9				
34·8	7·15	7·26	250·9				
45	7·2	7·26	250·9				
45·2	7·25	7·26	250·9	273	198·2	191·06	223
48·5	7·25	7·26	250·9				
55·5	7·3	7·26	250·9				
67·5	7·4	7·26	250·9				
77·7	7·3	7·26	250·9	271·95	196·7	191·15	246

TABLE XI.

$W_2 = 281.4.$ $W = 367.$ $R = 181.432.$ $c = 0.903.$

16·6	+ 4·8	......	200·71	180·2	214·75	155·23	250
28·3	5	......	200·53	180·1	214·35	155·43	260
39·7	5·1	......	200·45	179·9	214	155·45	270
49·2	5·15	......	200·4	179·7	213·8	155·37	278
53·5	5·05	5·15	200·4				
57	5·15	......	200·4	179·5	213·6	155·27	285
65·8	5·2	......	200·35	179·3	213·4	155·19	293
71·2	5·3	5·25	200·31				
76·5	5·2	5·25	200·31	179	213·1	155·09	303

TABLE XII.

$W_2 = 1029.7.$ $W = 1149.$ $R = 509.402.$ $c = 2.452.$

36·6	+12·7	......	564·35	462·5	442·1	551·88	369
45	12·8	......	564·1	462·1	441·2	552·13	380
51	12·8	......	564·1				
55	12·9	......	563·85	461·6	440·1	552·44	393
63	13	......	563·61				
70·3	13	......	563·61	460·7	438·6	552·65	411
78·6	13·2	......	363·12	460·1	437·5	553·59	423

TABLE XIII.

$W_2 = 574.$ $W = 621.$ $R = 175\cdot824.$ $c = 1\cdot166.$

$t.$	H.	H corrected.	$W_1.$	Vapour-volume. $v.$	Air-volume. $V.$	$\phi.$	Vapour-pressure. $p.$
$\circ$	millims.						
30·6	+ 94·4	saturated.					
33	still quite	saturated.					millims.
36·6	+105·9	......	183·5	389·8	134·1	644·41	512
45·2	106·2	......	183·15	390	133·2	644·96	526
55·6	106·6	......	182·7	390·25	132·1	646·02	544
64·6	106·9	......	182·33	390·45	131·1	647·28	560
67·4	106·9	......	182·33	390·4	130·9	646·98	564
79·4	107·3	......	181·9	390·6	129·7	648·40	583

TABLE XIV.

$W_2 = 730\cdot1.$ $W = 864.$ $R = 441\cdot582.$ $c = 1\cdot527.$

51·6	+ 24·85	......	338	389·9	200·1	890·28	741
61	24·9	......	337·92	389·5	199·3	892·04	765
72	25·05	......	337·7	389·3	198·2	895·61	794
80·7	25·2	......	337·47	389·1	197·3	898·57	817

TABLE XV.

$W_2 = 391\cdot9.$ $W = 316.$ $R = 537\cdot183.$ $c = 0\cdot853.$

54·3	+ 6·9	......	155·74	235·15	230·6	552·74	769
63	7·1	......	155·57	235·15	230·25	553·58	791
70·4	7·3	......	155·4	235·2	229·9	555·56	810
80·2	7·45	......	155·27	235·1	229·6	555·01	834
90·1	7·55	......	155·19	235	229·3	555·43	858

TABLE XVI.

$W_2 = 570.$ $W = 447.$ $R = 766\cdot94.$ $c = 1\cdot168.$

55·8	+65·5 {	just above saturation. }	143·48	425·85	264·2	1321·03	1020
62	65·7	......	143·25	426	263·7	1322·52	1010
71·5	66	......	142·9	426·2	263	1324·45	1061
74·5	66	......	142·9	426·2	262·85	1324·51	1080
88·2	66·4	......	142·43	426·5	261·8	1327·83	1125

Table XVII.

$W_2 = 719\cdot6.$ $W = 759.$ $R = 1709\cdot13.$ $c = 1\cdot656.$

$t.$	H.	H corrected.	$W_1.$	Vapour-volume. $v.$	Air-volume. $V.$	$\phi.$	Vapour-pressure. $p.$
$\overset{\circ}{70\cdot8}$	millims. $+10\cdot3$	covered.					millims.
$71\cdot7$	$13\cdot05$	{ without a breath. }	$410\cdot18$	$305\cdot4$	$367\cdot5$	$1431\cdot88$	1616
$77\cdot5$	$13\cdot05$	......	$410\cdot18$	305	$367\cdot2$	$1430\cdot97$	1644
$83\cdot6$	$13\cdot45$	$13\cdot4$	$409\cdot6$	$305\cdot3$	$366\cdot4$	$1435\cdot59$	1677
84	$13\cdot35$	$13\cdot4$	$409\cdot6$				
$89\cdot5$	$13\cdot5$	......	$409\cdot44$	$305\cdot1$	$365\cdot9$	$1436\cdot49$	1706
$96\cdot1$	$13\cdot9$	......	$408\cdot77$	$305\cdot4$	$364\cdot9$	$1447\cdot93$	1743

Table XVIII.

$W_2 = 669.$ $W = 724.$ $R = 1525\cdot47.$ $c = 1\cdot525.$

$83\cdot6$	$+76\cdot8$	{ strongly covered. }					
$84\cdot4$	$78\cdot6$	......	306	$359\cdot7$	246	$2309\cdot63$	2295
$84\cdot9$	$78\cdot6$	......	306				
$87\cdot6$	$78\cdot55$	$78\cdot6$	306				
$89\cdot5$	$78\cdot6$	......	306	$359\cdot5$	$245\cdot7$	$2309\cdot97$	2329
$90\cdot8$	$78\cdot6$	......	306	$359\cdot4$	$245\cdot6$	$2309\cdot95$	2338
$96\cdot8$	$78\cdot6$	......	306				
$97\cdot3$	$78\cdot6$	......	306	$359\cdot2$	$245\cdot2$	$2310\cdot95$	2382

Table XIX.

$W_2 = 425.$ $W = 487.$ $R = 874\cdot93.$ $c = 0\cdot938.$

88	$+73\cdot7$	{ strongly covered at the top. }					
89	$76\cdot75$	......	$193\cdot45$	$229\cdot3$	$127\cdot6$	$1620\cdot89$	2559
$91\cdot2$	$76\cdot8$	......	$193\cdot4$	$229\cdot3$	$127\cdot5$	$1621\cdot86$	2576
98	$77\cdot1$	......	$193\cdot12$	$229\cdot46$	$126\cdot92$	$1629\cdot48$	2635

The preceding Tables show that in the individual series of observations the vapour-volumes were approximately constant. Heated in these constant spaces, the vapour shows for the lower pressures, up to about 300 millims., constant values of ϕ, while for higher pressures ϕ increases in a slight degree with rising temperature; but in these cases the augmentation of ϕ is decidedly spread uniformly over the whole interval of the set of observations, and is by no means more prominent in the first degrees of the superheating. In several series it is evident that immediately from still observed saturation onwards, only just

this small alteration of ϕ makes its appearance. The following Table gives the variations of ϕ in percentages of the then subsisting value of ϕ for the lowest temperature. The variations are taken from the temperature- and respective pressure-intervals, and are calculated for $10°$ of further heating. In some cases, in which somewhat different values of $\frac{\Delta\phi}{\Delta t}$ appeared to result for the first part of the observations, the same expression $\frac{100}{\phi} \cdot \frac{10\Delta\phi}{\Delta t}$ was calculated separately for this part, and the numbers are enclosed in brackets in the Table. But it will be seen that in four instances out of the five this value is smaller than that derived from the entire interval of the observations ; so that the above assertion can decidedly be maintained. If in isolated places a value of H different by $0\cdot1$ millim. were taken (and that, according to what has been said, must be conceived to be the limit of the errors of observation here possible), the course of the values of ϕ would appear perfectly regular in the given direction.

TABLE XX.

Limits of pressure.	Limits of temperature.	$\frac{100}{\phi} \cdot \frac{10\Delta\phi}{\Delta t}$.
millims.		
123– 149	$27\cdot2$–$88\cdot2$	0
134– 178	$11\cdot2$–89	$+0\cdot12$
153– 199	$12\cdot6$–$94\cdot7$	0
166– 208	$9\cdot3$–$79\cdot3$	0
(„ – 192	„ –$53\cdot6$	$-0\cdot06$)
197– 240	$16\cdot6$–$76\cdot5$	0
203– 246	$17\cdot6$–$77\cdot7$	0
250– 303	$16\cdot6$–$76\cdot5$	0
369– 423	$36\cdot6$–$78\cdot6$	$+0\cdot07$
(„ – 411	„ –$70\cdot3$	$+0\cdot04$)
512– 583	$36\cdot6$–$79\cdot4$	$+0\cdot15$
741– 817	$51\cdot6$–$80\cdot7$	$+0\cdot32$
(„ – 794	„ –72	$+0\cdot29$)
769– 858	$54\cdot3$–$90\cdot1$	$+0\cdot14$
(„ – 810	„ –$70\cdot4$	$+0\cdot32$)
1020–1125	$55\cdot8$–$88\cdot2$	$+0\cdot16$
1616–1743	$71\cdot7$–$96\cdot1$	$+0\cdot3$
(„ –1706	„ –$89\cdot5$	$+0\cdot18$)
2295–2382	$84\cdot4$–$97\cdot3$	$+0\cdot05$
2559–2635	89 –98	$+0\cdot59$

Before further discussing the results, the observations on the two other vapours may be added. On the first of these, chloroform, from the small quantity of my material, I could only execute four series. The numbers are contained in Tables XXI.–XXIV.

Chloroform.
TABLE XXI.

$W_2 = 659\cdot4.$ $W = 673.$ $R = 325\cdot307.$ $c = 1\cdot605.$

t.	H.	H corrected.	W_1.	Vapour-volume. v.	Air-volume. V.	ϕ.	Vapour-pressure. p.
$\overset{\circ}{38}$	millims. +25·2	} decidedly					
41·2	37·1	} saturated.					
42·2	37·9	{ still a { breath.					millims.
42·9	38	clear.	325·16	332·4	309·55	389·33	370
45·3	38·2	......	324·84	332·6	309·1	389·95	373
48·8	38·25	......	324·76	332·5	308·8	389·80	377·5
58·6	38·6	......	324·2	332·7	307·85	390·29	389
70	39	......	323·56	332·9	306·7	390·95	403
79·8	39·25	......	323·16	332·85	305·8	391·11	415
90	39·65	......	322·52	333·05	304·6	392·07	427

TABLE XXII.

$W_2 = 438\cdot6.$ $W = 432.$ $R = 606\cdot982.$ $c = 1\cdot275.$

62	− 9·9	saturated.					
63	− 8·9	......	258·39	177·9	263·7	404·77	765
64	− 8·9	......	258·39	177·9	263·65	404·86	767
68·1	− 8·8	......	258·26	177·9	263·45	405·29	777
80·1	− 8·4	......	257·75	177·95	262·65	407·01	807
90·1	− 8·15	......	257·43	177·9	262·1	407·99	833

TABLE XXIII.

$W_2 = 549\cdot5.$ $W = 494.$ $R = 1231\cdot89.$ $c = 1\cdot259.$

73·8	−20	{ much satu- { rated.					
74·8	−14·6						
75·4	−11·25	{ appear- { ance of a { breath, { but un- { certain.	313·11	233·15	367·15	774·75	1158
75·9	−11·05	clear.	312·86	233·4	366·9	776·26	1160
79·2	−11	......	312·8	233·3	366·75	776·35	1172
94·9	−10·3	......	312·41	233	366	777·61	1228

TABLE XXIV.

$W_2 = 498.$ $W = 485.$ $R = 1226\cdot468.$ $c = 1\cdot18.$

90	+58·4	saturated.					
91·6	59	......	251·61	243·3	261·8	1179·16	1767
92·8	59·05	......	251·55	243·35	261·7	1179·75	1773
94·7	59·2	......	251·37	243·45	261·5	1181·00	1784
97·3	59·3	......	251·25	243·5	261·3	1182·35	1798
98	59·35	......	251·19	243·5	261·2	1182·22	1801

The proportions here are exactly the same as with sulphide of carbon. The following Table XXV. corresponds to Table XX. (for sulphide of carbon).

TABLE XXV.

Limits of pressure.	Limits of temperature.	$\dfrac{100}{\phi} \cdot \dfrac{10\Delta\phi}{\Delta t}$.
millims.		
370– 427	$42°\!\cdot\!9$–$90°$	+0·15
(„ – 415	„ –79·8	+0·12)
765– 833	63 –91	+0·29
(„ – 777	„ –68·1	+0·25)
1158–1228	75·4–94·9	+0·2
(1160– „	75·9– „	+0·09)
1767–1801	91·6–98	+0·4

With respect to the variation shown by the two numbers taken from Table XXIII., the remark there made in the column " H corrected " may serve to explain it.

Things assume a different form with alcohol; for here phenomena of adhesion are very distinctly recognizable. For example, the following are the numbers, arranged in the order of time, of one series of observations, together with the notes taken from my journal :—

TABLE XXVI.

t.	H.
	millims.
$98°\!\cdot\!5$	−2·65
95·8	−2·85

At 95°, on the alcohol side a very feeble breath seems already to form on the glass, of condensed particles of vapour.

	millims.
$92°\!\cdot\!9$	−3·15
90	−3·65
88·2	−7·1 decidedly saturated.
90·1	−3·55
93·2	−3·25

During the heating from 90°·1 to 93°·2 a narrow dull ring is seen to have formed round the uppermost layers of the mercury on the alcohol side, proceeding from vapour-particles precipitated during the previous cooling from 95° to 90°, which are still retained. During the continued heating, gradually larger vesicles of vapour are seen to form there, consequently below the level of the mercury.

	millims.
95·1	−2·95
97·1	−2·95
97·6	−2·95

There is now evidently a somewhat smaller quantity of vapour in action than at the commencement of the experiments; and this is recognizable in the feebler tension of the vapour.

The influence of this adhesion shows itself also in the series of the following Tables for alcohol. I have on each occasion been able to convince myself of this by simply observing the tubes narrowly: to a practised eye the difference in behaviour between such an adhering substance and one that does not adhere, in the vicinity of saturation and in the transition to saturation, is not to be mistaken. It only remains surprising that in my earlier investigations I found alcohol not to be one of the number of adhering substances. It may, however, be remarked that then and now I investigated two different preparations, and it is quite conceivable that the slightest shade of difference in preparing the material may call forth this property. Tables XXVII.–XXXIII. contain the observations on

Alcohol.

TABLE XXVII.

$W_2 = 597 \cdot 1.$ $W = 668.$ $R = 51 \cdot 465.$ $c = 1 \cdot 2545.$

t.	H.	H corrected.	W_1.	Vapour-volume. v.	Air-volume. V.	ϕ.	Vapour-pressure. p.
° 20	{ considerable nega- tive H.						
36·2	+39·7	saturated.					
38	45·1						
39·5	48·7						millims.
43	52·3	......	277·98	317·6	204·7	132·41	132
52·5	53·5	......	276·48	318·75	202·6	133·36	136
64·5	55·6	......	273·85	321	199·3	135·77	143
82·5	56·4	......	272·85	321·35	197·2	135·84	149

TABLE XXVIII.

$W_2 = 700 \cdot 5.$ $W = 761.$ $R = 299 \cdot 388.$ $c = 1 \cdot 633.$

t.	H.	H corrected.	W_1.	Vapour-volume. v.	Air-volume. V.	ϕ.	Vapour-pressure. p.
54·6	negative.	{ much sa- turation.					
58·6	+7·5	......	381·59	315·9	318·1	304·46	320
60	7·8	......	381	316·3	317·6	305·57	322
67·3	8·4	... ..	380·12	316·9	316·15	307·72	330
70·5	8·4	......	380·12				
78·2	8·7	......	379·63	316 85	315·1	308·90	342
88	8·9	......	379·3	316·6	314·2	309·38	353
92·1	8·8	8·9	379·3				
98	8·9	......	379·3	316·1	313·7	309·26	363

TABLE XXIX.

$W_2 = 708.5.$ $W = 404.$ $R = 1143.34.$ $c = 1.706.$

t.	H.	H corrected.	W_1.	Vapour-volume, v.	Air-volume. V.	ϕ.	Vapour-pressure. p.
77·9	millims. +1	covered.					
78·8	4·5	{covered. {a trace.					millims.
79·5	8·1	......	227·44	479·15	530·75	1039·12	764
80·1	5	5·1	227·44				
80·7	5·2	......	227·27	479·3	530·55	1039·94	767·5
82	5·4	......	226·93	479·6	530·2	1041·52	771
84·4	5·8	......	226·25	480·25	529·5	1044·79	777
86	5·9	......	226 08				
87	6·1	......	225·73	480·7	528·9	1047·29	784
88·3	6·3	......	225·39				
90	6·4	......	225·22	481·15	528·3	1049·78	792
92·5	6·8	......	224·54				
93	6·8	......	224·54	481·8	527·6	1053·04	800
96·4	7·05	......	224·11				
98	7·15	......	223·94	482·25	526·9	1055·74	812

TABLE XXX.

$W_2 = 621.2.$ $W = 401.$ $R = 846.97.$ $c = 1.479.$

t.	H.	H corrected.	W_1.	Vapour-volume, v.	Air-volume. V.	ϕ.	Vapour-pressure. p.
78·7	+14·3	saturated.					
79·2	15·9	do.					
79·5	scarcely a breath.						
80·3	16·55	......	174·99	444·9	393·1	979·42	778
80·8	16·45	16·55	174·99				
82	16·75	......	174·69	445·15	392·75	980·97	782
83	16·95	......	174·4	445·4	392·4	982·57	785
84·8	17	......	174·32				
86	17·2	......	174·03	445·65	392	984·24	793
87·4	17·2	......	174·03				
88·9	17·6	......	173·44	446·3	391·3	987·72	801
90·7	17·8	......	173·14	446·6	390·95	989·39	805
93·8	18	......	172·84	446·8	390·55	990·89	813
97·2	18·2	......	172·55	447·1	390·2	992·45	826

TABLE XXXI.

$W_2 = 597.1.$ $W = 635.$ $R = 809.849.$ $c = 1.388.$

t.	H.	H corrected.	W_1.	Vapour-volume, v.	Air-volume. V.	ϕ.	Vapour-pressure. p.
81·5	−56·2	saturated.					
82·1	−54·8	......	372·35	220·4	331·7	504·10	812
82·4	−54·8	......	372·35				
83	−54·35	......	371·73				
84·4	−54·35	......	371·73	220·9	331·05	506·79	820
87·4	−54·1	......	371·38	221·1	330·6	508·42	829
89·3	−53·9	......	371·1	221·3	330·2	509·84	835
91·8	−53·8	......	370·96	221·3	330·0	510·45	841
97·7	−53·6	......	370·68	221·2	329·4	511·85	858

Table XXXII.

$W_2 = 685.$ $W = 400.$ $R = 1023\cdot76.$ $c = 1\cdot515.$

t.	H.	H corrected.	W_1.	Vapour-volume. v.	Air-volume. V.	ϕ.	Vapour-pressure. p.
°	millims.						
80·8	+12·7	{ much satu-rated.					
82·5	20·3	still a trace.					millims.
83	21·2	doubtful.					819
83·6	21·9	not a breath.	175·2	508·5	458·2	1167·37	820
84	21·9	......	175·2	508·5	458·15	1167·46	834
89·5	22·4	......	174·44	509·2	457·25	1171·54	853
96·1	23·3	......	173·08	510·5	455·7	1179·10	

Table XXXIII.

$W_2 = 597\cdot1.$ $W = 635.$ $R = 809\cdot849.$ $c = 1\cdot388.$

88·2	− 7·1	saturated.					
90	− 3·65	......	299·57	293·95	257·5	921·53	1138
90·1	− 3·55	......	299·43	294·05	257·35	922·46	1139
92·9	− 3·15	−3·2	298·95	294·5	256·75	926·34	1151
93·2	− 3·25	−3·2	298·95				
95·8	− 2·85	measured { during cooling.	298·46	294·8	256·1	929·95	1163
98·5	− 2·65		298·18	295	255·7	932·21	1174
95·1	− 2·95	measured { during heating.	298·6	294·7	256·3	928·82	1160
97·1	− 2·95		298·6				1169
97·6	− 2·95		298·6	294·7	256·15	929·38	

The values for $\dfrac{100}{\phi} \cdot \dfrac{10\Delta\phi}{\Delta t}$ are given in Table XXXIV.

Table XXXIV.

Limits of pressure.	Limits of tempe-rature.	$\dfrac{100}{\phi} \cdot \dfrac{10\Delta\phi}{\Delta t}$.
millims.	° °	
132– 149	43 –82·5	+0·47
320– 363	58·6–98	+0·4
(„ – 330	„ –67·3	+1·2)
(330– 363	67·3–98	+0·16)
(342– 363	78·2–98	0)
764– 812	79·5–98	+0·9
778– 826	80·3–97·2	+0·8
(785– „	83 – „	+0·7
812– 858	82·1–97·7	+0·99
(835– „	89·3– „	+0·47)
819– 853	83·6–96·1	+0·8
1138–1174	90 –98·5	+1·36
(1151– „	92·9– „	+1·13)

From a comparison of the numbers in brackets in this Table with the numbers standing above each (which correspond to a series of observations) it is obvious that the behaviour of alcohol vapour totally differs from that of the two other vapours studied. The values of $\frac{\Delta\phi}{\Delta t}$, near saturation, are decidedly greater for the first degrees of further heating, in consequence of adhesion; for the higher intervals of temperature they partially become considerably smaller. We see further that even in the little pressures between 132 and 149 millims., where in my earlier investigations[*] I found the vapour still entirely in the gaseous state, a high positive value of $\frac{\Delta\phi}{\Delta t}$ is shown. The phenomena of adhesion, therefore, here completely mask the behaviour of the vapour.

On this account, in our further discussion of the results obtained, the alcohol numbers must be altogether neglected. Where adhesion comes into play, there we have to do with variable quantities of vapour, and therefore with ratios utterly impossible to calculate.

This leads us to a remark upon the investigation of steam (in the same direction) by Fairbairn and Tate. These experimenters, when experimenting on the specific volumes of steam from the lowest point of saturation upwards[†], preferred small increments of heat (a few degrees Fahrenheit), and believed they had found coefficients of expansion of considerable magnitude; but in a subsequent memoir[‡] they traced the relations more accurately, since even to themselves the previous results did not appear sufficiently reliable.

They thus arrived at this result—that, from the limit of saturation, steam heated within a space of constant dimensions exhibits for the first two degrees a considerably higher coefficient of expansion than air, while from that point upwards the coefficient is the same for steam and for air. Translated into my formula of calculation, this would signify that ϕ increases considerably with the temperature for the first degrees, and then becomes constant. I will leave out of consideration the fact that the second part of this proposition is not strictly the expression of their observations, and merely point out, with respect to the first part, that in their experiments the initial pressures of the steam amounted to less than half an atmosphere. This corresponds to initial temperatures of at most a few more than 70 degrees Celsius; and at such temperatures, according to my earlier ex-

* Pogg. *Ann.* vol. cxxxvii. p. 28.   † Phil. Trans. 1860, p. 185.
‡ Phil. Trans. 1862, p. 591.

periments on steam*, adhesion always comes into play. Fair-
bairn and Tate's result is therefore itself quite intelligible, but
throws no light upon the exact behaviour of steam. Moreover
these experimenters would not possibly, by their method, have
attained any results even for not adhering vapours; for they fall
into the oft committed and as often exposed error of having the
upper part of the pressure-measuring mercury column in the
heated bath, and the lower part in the temperature of the room.

§ 5.

Confining ourselves, then, to the consideration of the values of
$\frac{\Delta\phi}{\Delta t}$ found for sulphide of carbon and chloroform, these hold, as
we have said, for an approximately constant vapour-space. Now
with low pressures the quotient $\frac{\Delta\phi}{\Delta t}$ appears $=0$; that is, there
the superheated vapour experiences exactly the same pressure-
expansion for constant volume as does the dry air with which
it was compared. Hence the question mentioned at the com-
mencement, and started by my earlier experiments, is now de-
cided thus—that the superheated vapour under small pres-
sures can certainly possess smaller coefficients of expansion for
constant volume than 0·003663, so far as such smaller coefficients
can belong also to dry air, but that a still smaller expansion of
vapour is not to be admitted. According to Regnault†, how-
ever, dry air under these small pressures shows decidedly
smaller coefficients than 0·003663—for example, with an initial
pressure of 110 millims. the value 0·003648; so that my
former results (which were directed only to comparison with
the number 0·003663) are certainly hereby confirmed, and at
the same time have the limits of their validity prescribed in the
behaviour of dry air.

For higher pressures (that is, where at the boundary of satu-
ration, according to my former experiments, vapour shows a
considerable departure from the gaseous state) the quotient
$\frac{\Delta\phi}{\Delta t}$ obtains truly a small, but yet an undeniable positive value.
Consequently the pressure-expansion of vapour contained within
a constant volume is there greater than that of dry air; and
indeed we shall not be wrong if we suppose that, the greater
the vapour-pressure becomes, so much greater is the positive
value of $\frac{\Delta\phi}{\Delta t}$. This is sufficiently indicated by Tables XX. and

* Pogg. *Ann.* vol. cxxxvii. p. 602.
† *Mém. de l'Acad.* vol. xxi. p. 110.

XXV.; and with this my earlier experiments are in accord : it must be remembered that the variations of vapour-density there found are, in their significance, inversely proportional to the variations of ϕ which we have now obtained.

It results further from the present observations (which often extended over a wide interval) that this slight variation of ϕ is by no means confined to the first two degrees of superheating above the point of saturation, but is, on the average, uniformly distributed over the entire interval. Of course this is to be understood thus—that the gradual transition of ϕ to its constant maximum value, the occurrence of which when the heating is continued within a constant space has just been made certain by our present experiments, only ensues very slowly. It is true that the positive value of $\frac{\Delta\phi}{\Delta t}$ will diminish and finally become *nil*, but the latter only with a very extreme degree of superheating.

The lower limit to which the superheating must certainly be carried, in order to arrive at constant values of ϕ, can be approximately fixed. For example, for a temperature of saturation of 90°, according to my previous formula the purely saturated vapour would have a density equal to $0.0595 \sqrt{273 + 90}$ or 1.13 the constant density finally attained in the gaseous state. It is true that, for sulphide of carbon and chloroform, this formula was not pursued quite up to that temperature; but at 50° the ratio 1.07 was directly found; so that, according to all the experience then acquired, the above number for 90° is certainly not far from correct. Accordingly, for purely saturated vapour, the value of the function ϕ would be less than the constant value to which ϕ continually more and more approaches, in the ratio of $1 : 1.13$. If now such vapour is heated in a constant volume, for every 10 degrees of superheating ϕ increases at the commencement, according to Tables XX. and XXV., to about 1.004 of its value. And since the value of $\frac{\Delta\phi}{\Delta t}$, in accordance with what has been observed, must subsequently become smaller, we may therefore say that, in order to attain the final constant value of ϕ, a superheating of more than $\frac{130}{4}.10$ degrees, or more than 325°, is here required.

In like manner for an extreme temperature of 50°, at which the total deviation of ϕ amounts to 1.07 its final value, Tables XX. and XXV. indicate on the average an initial increase of $\frac{\Delta\phi}{\Delta t}$ to 1.002 for 10° superheating. In this case, therefore, for

the attainment of the gaseous state a greater superheating is required than $\frac{70}{2}$. 10 or 350°.

We thus see that, wherever a sensible deviation of purely saturated vapour from the gaseous state has place, a very considerable elevation of temperature is necessary in order by superheating in constant volume to make it pass into the gaseous condition.

For practical application we hence obtain the not unimportant result, that, at least so long as the limits of these experiments are not exceeded (therefore to about 4 atmospheres pressure), only trifling errors are entailed by putting the coefficient of pressure-expansion for a constant volume simply equal to the coefficient of expansion of air, provided the limits of the temperatures under consideration are not too wide, say not more than 50°.

For the theoretical side of the question, however, such a trifling augmentation of ϕ is perfectly sufficient to cause the proportions to appear quite different from what they would be with ϕ constant.   To see this it is best to keep in view the two directions in which I have followed the variability of the vapour-densities.   The earlier experiments give the variableness which occurs when at a constant temperature the vapour expands its volume; the present when, the volume remaining constant, the temperature is raised.   In both ways the gaseous state is gradually reached.   Now in the latter this takes place with singular slowness.   Nevertheless we can as yet only say further that, in the cases discussed, fully twice the absolute temperature of the vapour is necessary.   Looking at the variations occurring when the first way is taken, my earlier experiments show that in individual cases an increase of the volume to four or five times may be required in order to attain the gaseous state.

Accordingly, if in

$$pv = \phi(273 + t)$$

we regard ϕ as a function of v and t, we have the partial differential quotients $\left(\frac{d\phi}{dv}\right)_t$ for constant temperature and $\left(\frac{d\phi}{dt}\right)_v$ for constant volume to be equally taken account of, and must not neglect the latter on account of the small values of $\frac{\Delta\phi}{\Delta t}$ which have been obtained.

Lastly, the numerical value of the coefficient of pressure-expansion of vapours, which is not so directly to be learned from the variations of ϕ, may be fixed, and compared with the numbers given by Regnault for gases, in order to obtain points of support on this side also.   For the coefficient of pressure-expan-

sion with constant volume we have the well-known general formula

$$\alpha = \frac{\left(\frac{dp}{dt}\right)_v}{p - t\left(\frac{dp}{dt}\right)_v}.$$

Now, from the formula introduced for vapours

$$pv = \phi(273 + t),$$

we obtain for constant volume

$$\left(\frac{dp}{dt}\right)_v = \frac{273 + t}{v}\left(\frac{d\phi}{dt}\right)_v + \frac{\phi}{v}.$$

Therefore for vapours

$$\alpha = \frac{(273 + t)\left(\frac{d\phi}{dt}\right)_v + \phi}{pv - (273 + t)\left(\frac{d\phi}{dt}\right)_v t - \phi t};$$

that is, considering that $pv = \phi(273 + t)$,

$$\alpha = \frac{1 + \dfrac{273 + t}{\phi}\left(\dfrac{d\phi}{dt}\right)_v}{273 - \dfrac{273 + t}{\phi}\left(\dfrac{d\phi}{dt}\right)_v t}.$$

When $\left(\dfrac{d\phi}{dt}\right)_v = 0$, this value becomes $\dfrac{1}{273} = 0.003663$; for positive quotients of $\left(\dfrac{d\phi}{dt}\right)_v$ it is greater. Putting, for example, $t = 90°$, and, as before, $\dfrac{1}{\phi}\left(\dfrac{d\phi}{dt}\right)_v = 0.0004$ (corresponding to Tables XX. and XXV., of which the values $\dfrac{100}{\phi} \cdot \dfrac{10\Delta\phi}{\Delta t}$ are now to be divided by 1000), we get

$$\alpha = \frac{1 + 0.1452}{273 - 0.1452 \cdot 90} = 0.0044.$$

Under a pressure of about three atmospheres, to which in the case of sulphide of carbon this number would belong, Regnault[*] finds for carbonic acid the coefficient of pressure-expansion 0.0038. For carbonic acid, according to the formula

[*] *Mém. de l'Acad.* vol. xxi. p. 112.

(equivalent to the above)

$$\frac{1}{\phi}\left(\frac{d\phi}{dt}\right)_v = \frac{273\alpha - 1}{(1 + \alpha t)(273 + t)},$$

putting $\alpha = 0{\cdot}0038$, we should obtain

$$\frac{1}{\phi}\left(\frac{d\phi}{dt}\right)_v = 0{\cdot}00008,$$

or, corresponding to the values of the Tables,

$$\frac{100}{\phi} \cdot \frac{10\Delta\phi}{\Delta t} = 0{\cdot}08.$$

Accordingly sulphide of carbon shows in this case only about five times the variableness of vapour-density shown by carbonic acid gas.

Similarly, for the temperature of 50°, using the mean values of the Tables for this temperature

$$\frac{100}{\phi} \cdot \frac{10\Delta\phi}{\Delta t} = 0{\cdot}2,$$

with sulphide of carbon

$$\alpha = 0{\cdot}00395.$$

We see that in all there are perfectly comparable deviations from the gaseous laws to which, in this direction, sulphide of carbon and carbonic acid are subject.

On the second coefficient to be mentioned in connexion with vapours, viz. the coefficient of space-expansion with constant pressure, after the present results at least something general must be said. For it the general formula holds :—

$$\alpha = \frac{\left(\frac{dv}{dt}\right)_p}{v - t\left(\frac{dv}{dt}\right)_p},$$

which, when

$$pv = \phi\,(273 + t)$$

becomes

$$\alpha = \frac{1 + \dfrac{273 + t}{\phi}\left(\dfrac{d\phi}{dt}\right)_p}{273 - \dfrac{273 + t}{\phi}\left(\dfrac{d\phi}{dt}\right)_p t}.$$

When, now, from a determined initial point, ϕ is traced for heating in constant volume, we arrive at a higher final expression with the final temperature than when we reach the same temperature, after starting from the same point, under constant

pressure. With constant temperature, however, according to my earlier experiments, ϕ augments as the pressure diminishes. Consequently for the same limits of temperature

$$\left(\frac{d\phi}{dt}\right)_p > \left(\frac{d\phi}{dt}\right)_v.$$

Hence the value of α is universally greater for constant pressure than for constant volume.

Physical Laboratory of the Polytechnic,
    Aachen, June 5, 1872.

LIII. *On Duplex Telegraphy.* By Oliver Heaviside, *Great Northern Telegraph Company, Newcastle-on-Tyne**.

[With a Plate.]

DUPLEX TELEGRAPHY, the art of telegraphing simulta-neously in opposite directions on the same wire, which was first performed by Dr. Gintl in 1853, and subsequently engaged the attention of so many inventors, until lately seemed never likely to be carried out in practice to any extent. According to the very practical author of ' Practical Telegraphy,' " this system has not been found of practical advantage ;" and if we may believe another writer, the systems he describes " must be looked upon as little more than feats of intellectual gymnastics—very beautiful in their way, but quite useless in a practical point of view." However, notwithstanding these unfavourable reports as to the practicability of duplex telegraphy, the experience of the last year has negatived them in a striking manner, and made the so-called " feats " very common-place affairs. Circuits worked on a duplex system are now established in various parts of the United Kingdom—not to mention the United States, where the resurrection of these defunct schemes took place—and continue to give every satisfaction. There seems little reason to doubt that this system will eventually be extended to all circuits of not too great a length, between the terminal points of which there is more than sufficient traffic for a single wire worked in the ordinary manner—that is to say, only one station working at a time.

I propose in this paper to give a short account of the theory of duplex telegraphy by the principal methods, and to describe two other methods, which are, I believe, entirely original.

To begin at the beginning. Prior to 1853, it is said to have been the current belief of those best qualified to judge, that to send two messages in opposite directions at the same time on a

* Communicated by the Author.

Fig. 1.

Station A.   Station B.
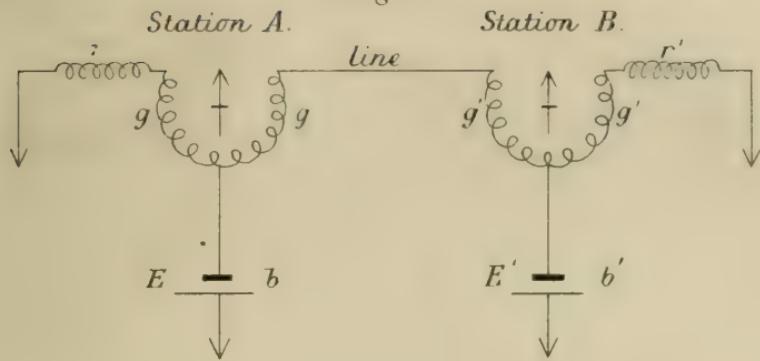
Fig. 2.
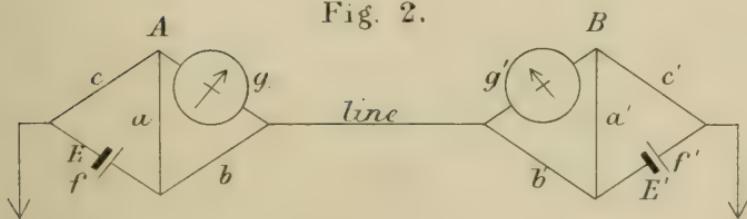
Fig. 3.
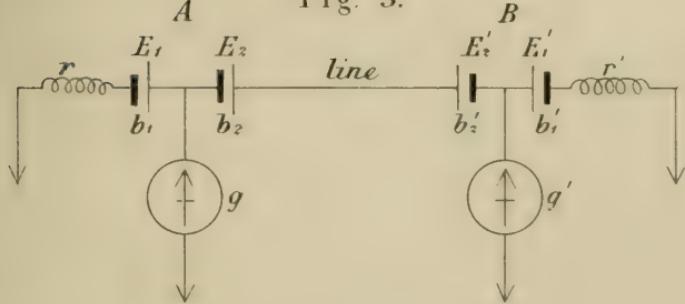
Fig. 4.
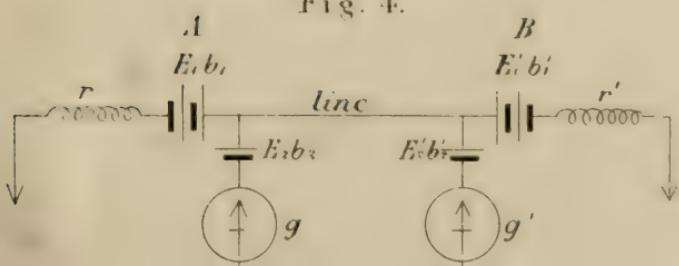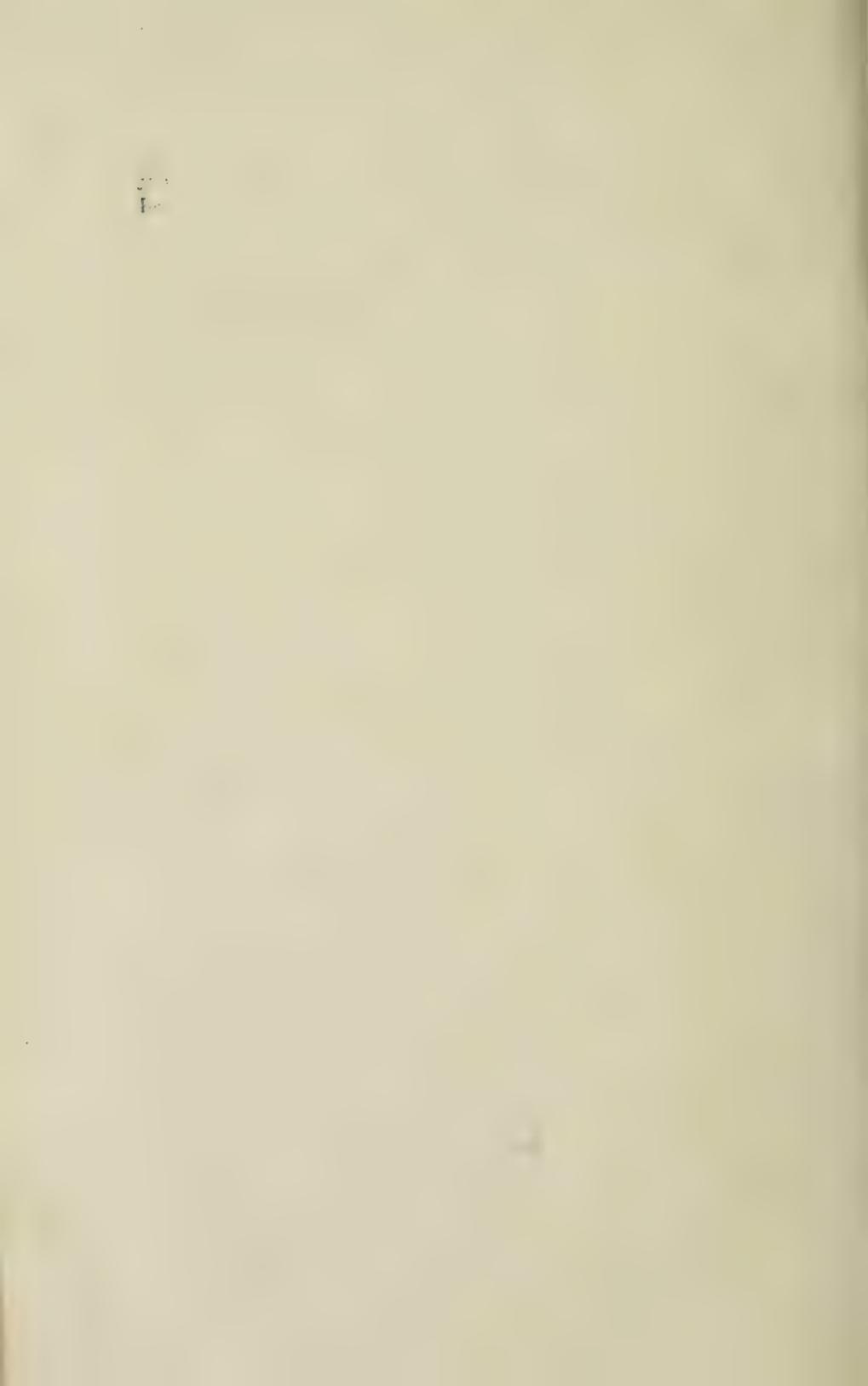
single wire was an impossibility; for it was argued that the two
messages, meeting, would get mixed up and neutralize each other
more or less, leaving only a few stray dots and dashes as irvi-
vors (after the manner of the Kilkenny cats, who devoured one
another and only left their tails behind).   However, Dr. Gintl
effectually silenced this powerful argument by going and doing it.

In order to be able to receive messages from another station,
it is necessary for the receiving instrument to be in circuit with
the line; and in order to send to another station, the battery
must be in circuit.   Hence, in order to receive and send at the
same time, both the sending and receiving apparatus must be in
circuit together.   This can be arranged by making one continuous
circuit between the two earths, and including the line and all the
apparatus at each station.   But if nothing further were done, the
receiving instruments would be worked both by the received and
sent currents; and if both stations worked at once, inextricable
confusion would be the only result.   Now, evidently, if the effect
of the sent currents on the sending-station's instrument can be
neutralized, the " feat " is accomplished.   There are many ways of
doing this.   Dr. Gintl surmounted the difficulty in what was, to say
the least, a very ingenious manner, although, from a modern point
of view, it was decidedly clumsy.   He made his key, while being
depressed to send a current to the line through his own relay,
at the same time close a local circuit, including a coil of wire
outside the principal coils of the relay, in such a manner that
the current in this local circuit (which contained an independent
battery) circulated round the cores of the electromagnets in the
opposite direction to the current going out to the line; and by
placing a rheostat in this local circuit he was able to vary the
strength of the local current, so that the effect of the out-going
current on the relay was exactly neutralized.   The relay then
responded only to currents coming from the opposite station,
which, of course, passed through the inner coils alone.   Did
both stations depress their keys simultaneously, the current in
the batteries, inner coils, and the line was that due to both bat-
teries; but in each relay as much of this current as was due to
the corresponding battery was neutralized by the local current.
The line-current might even be nothing, which would happen if
each station had equal batteries and the same poles to earth.
Then the relays would be worked entirely by the local current.

But local circuits are nuisances, and it is not to be wondered
at that this method of Gintl's never came into practical use.
But the possibility of the " feat " having been once demon-
strated, it was not long before another and much superior me-
thod was introduced.   It was discovered about the same time in
1854 by Frischen and Siemens-Halske, and may be called the

differential method. It is represented in its simplest form in Plate VII. fig. 1. The relay at each station is wound with two coils of equal length; and the connexions with the battery and the line are made in the same manner as if each station were taking a test of the resistance of the line with the differential galvanometer. The resistance r, then, at station A equals the whole resistance outside station A; and r' at station B equals the whole resistance outside station B. Then, when the battery E is in circuit, as its current divides equally between the two coils of the instrument $g\,g$, the latter is unaffected; but that half of the current which passes to the line necessarily influences the instrument $g'\,g'$ at the other station, since the whole of it passes through one coil, and then divides between the other coil and the battery E'. Thus each station does not work its own relay, but only that of the opposite station, and the conditions of duplex working are satisfied.

It is upon this system that nearly the whole of the existing methods of duplex telegraphy are founded. As the object is to prevent out-going currents from working the sending-station's instrument, it is plain that there may be many modifications having for object the easier production of balances under different circumstances—as by varying the distance of one or both coils from the armature instead of altering the resistance r (fig. 1). There are also a few small points to be attended to before this system can be considered perfect. First, it is necessary for the external resistance to be as constant as possible, in order that the currents sent by a station (say A) may never affect its own instrument. But this external resistance includes B's apparatus; and B's battery is sometimes in and sometimes out of circuit. A variation in the external resistance will therefore be caused unless the transmitting apparatus is so arranged that a resistance equal to that of the battery is substituted for it when the latter is not in circuit. Again, there should be no interval of time during which neither the battery nor this equivalent resistance is in circuit. These things can generally be arranged with little difficulty. Thus, taking the case of the simplest transmitting instrument (the common Morse key), consisting of merely a lever with a front and back contact, the equivalent resistance may be connected with the back, and the battery-pole with the front contact; and the interval of disconnexion may be avoided by the use of suitable springs, or other means, by which the front contact is made just before (or practically at the same time as) the back contact is broken, and *vice versâ.* There will then be only a very much smaller interval of time during which the received currents can pass both through the battery and its equivalent resistance. The application of this to more compli-

cated instruments (as, for instance, Wheatstone's automatic transmitter) is not at first sight so evident; but I have done it in a very simple manner, which it is unnecessary to describe. On long lines, or with high-speed instruments, an attention to these *minutiæ* is desirable; but on short lines and with common Morse apparatus they are superfluous.

It is not essential, though sometimes desirable, to use differentially wound instruments. Most telegraph instruments are constructed with two separate coils of wire, each on its own core. By connecting the battery to the wire joining these coils, we have a differential arrangement, and frequently all that is needed. In fact, if the armature is polarized, as in most relays, the result is the same as if they were differentially wound. With an unpolarized Morse direct-writer, however, the effect of the outgoing currents would not be completely neutralized. This is of little consequence, as the spring which draws the armature from the electromagnets may have a tension given it that only the received currents can overcome. The rheostats r and r' (fig. 1) may even be dispensed with and a direct earth-connexion substituted, provided the external resistance be not too great.

Quite recently another system has been brought forward, undeniably the most perfect, which may be called the *bridge duplex*, its principle being that of Wheatstone's bridge. To whom the idea first occurred of using this arrangement for duplex telegraphy is unknown to me. It has been claimed by Mr. Eden, of Edinburgh; but it has been patented by Mr. Stearns, of Boston, U.S., who also patents a number of plans, all depending on the differential system before described.

The arrangement for the bridge duplex is shown theoretically in fig. 2. a, b, c and a', b', c' are resistances, g and g' the receiving instruments, and f and f' the batteries. By the well-known law of the balance, when $a : b = c : d$, where d is the whole external resistance between station A and the earth at B, the electromotive force E will cause no current in g; and similarly for station B. The circumstance that the out-going currents do not pass through the receiving instruments is very important, as it allows any description of existing instruments to be used, and without any alteration. As in the differential plan, it is not always indispensable to adhere rigidly to the conditions which give theoretical perfection.

Although the signals sent by station A are only received at B, and *vice versâ*, and it is convenient to assume that the currents producing these signals actually come from the opposite station, yet it does not always happen that such is the fact. To take an extreme case. Let all the apparatus at each station, and

likewise the batteries, be exactly alike, and the line of uniform insulation. Now let A depress his key. The galvanometer at B will be deflected, but not A's. Let now B depress *his* key. No change will be produced in the deflection of B's galvanometer; but A's will be deflected. But if A and B have both the same pole of their batteries to line, there will be no current in the line, which, by Bosscha's first corollary, may be removed without producing any alteration in the currents in the remaining circuits. It is thus evident that A and B are both working *their own* instruments; and this supplies us with a very easy way of calculating the strength of the received signals—which would otherwise be very complicated. We have only to consider the current produced in $(g+b)$ by E having resistance f, with an external resistance c, $(g+b)$ being shunted by a resistance a, and we find the strength of the signal to be

$$G = \frac{Ea}{(f+c)(a+b+g) + a(b+g)}.$$

I will now describe two original methods of duplex working, which though perhaps not quite so easily put into practice as the foregoing, may be interesting from the theoretical point of view.

As in the bridge arrangement, the out-going currents do not pass through the receiving instruments at all. In the first of these plans, as shown in fig. 3, the receiving instrument at each station is connected between the middle of two batteries and the earth. r and r' are rheostats. The condition that the batteries E_1 and E_2 at station A cause no current in g is that

$$E_1 : E_2 = r + b_1 : b_2 + d,$$

d being the exterior resistance as before; and similarly for the other station. The strength of current sent to line is $\dfrac{E_1}{r+b_1}$ or $\dfrac{E_2}{b_2+d}$. In the second plan, shown in fig. 4, one of the batteries at each station (E_2 and E'_2) is placed in the same branch as the receiving instrument, and both E_1 and E_2 tend to send the same current to the line. When $E_1 : E_2 = 1 + \dfrac{r+b_1}{d}$ (d having the same meaning as before), the electromotive force E_2 is neutralized and there is no current in g. The line-current is $\dfrac{E_1}{r+b_1+d}$.

I have adapted these plans to the direct-writing Morse by using an ordinary reversing key (which is nothing more than two keys insulated from each other and worked by the same lever), in order to put the two batteries E_1 and E_2 simultaneously in or out of circuit. I also found it necessary for there to be no in-

terval of disconnexion, but did not find it necessary to introduce
equivalent resistances when they were out of circuit, though
theoretically this should be done, and the same key could be
made to do it.

The last plan will be easily recognized to be based on the
method of comparing electromotive forces known as Poggen-
dorff's compensation, in which the battery having the lesser
electromotive force is not allowed to act. The other plan (fig. 3)
is also an adaptation of a method of comparing the *working*
electromotive forces of batteries, which I devised three years ago,
and subsequently published in the 'English Mechanic' for July
5, 1872, No. 380, p. 411. My only reason for mentioning this
is, that it is claimed by Emile Lacoine as a " new method of
determining voltaic constants," in the *Journal Télégraphique*,
vol. ii. No. 13.   See also the 'Telegraphic Journal' for April
this year.

The greatest drawback to duplex working (and this is common
to all known systems) is the changeability in resistance of the
line-wire itself, caused by defective insulation, variations of tem-
perature, &c. ; and in such a wet and changeable climate as ours,
this fixes a limit to the length of line on which a duplex system
can be worked with advantage, making it less than can be worked
through in the ordinary manner. On short lines the resistance never
varies much in any weather (unless actual faults occur), and it is not
necessary to vary the balancing resistances. But on long lines this
variation is sometimes very considerable ; and it is questionable
whether, in the present state of telegraphy, a long circuit in this
country, as from Glasgow to London, could be profitably worked
in wet weather. But the variations in the resistance of submarine
cables (having no land-lines attached) are so very much less,
that it seems probable, *à priori*, duplex telegraphy would be suc-
cessful with them. Of course their electrostatic capacity must
be balanced by condensers. It could also be applied to the
system by which some long cables are worked, where there is
no metallic circuit through the receiving instrument, which is
placed between a condenser and the cable.

Those systems where the out-going currents do not pass
through the receiving instruments have a peculiar and perhaps
what will some day (when telegraphy, now in its infancy, has
arrived at years of discretion) be considered an important ad-
vantage over the differential system.   It is theoretically possible
to send any number of messages whatever simultaneously in one
and the same direction on a single wire.   Now by combination
with a "null" duplex system it becomes obviously possible to
send any number of messages in the other direction while the
opposite correspondences are going on, and without interference.

Thus the working capacities of telegraphic circuits may be increased indefinitely by suitable arrangements. Practically, however, it would seem that a limit would soon be reached, from the rapidly increasing complication of adjustments required. Besides, to keep them going, the telegraph-clerks must themselves be electricians of a rather higher order than at present; and, considering the condition of the labour-market and the youth of the school-boards, that would scarcely pay. Nevertheless, from experiments I have made, I find it is not at all a difficult matter to carry on *four* correspondences at the same time, namely two in each direction; and if we may suppose the growth of telegraphy will be as rapid in the future as it has been in the past, it seems not improbable that *multi-telegraphy* will become an established fact.

In a following paper I intend giving the formulæ necessary for calculating the proper proportions of the resistances &c. to suit different lines and apparatus, so that the greatest possible amount of current may be driven through the receiving instruments, where alone it is of practical service.

April 19, 1873.

LIV. *On the Theory of the Normal Magnet, and the Means of augmenting indefinitely the Power of Magnets.* By J. JAMIN[*].

IN the sitting of the 16th December, 1872, I made known to the Academy the process by which I was enabled to measure the force necessary to separate the same very small iron contact placed on different parts of a magnet. This force is measured in grammes; I designate it by F. I will now state how it varies for the different points in a magnetized plate which is straight, long, flat, and broad.

On the line drawn in the middle of the plate, parallel to its length—that is to say, along the axis, F = 0, not only in the centre, but to within a short distance of the two ends, after which it increases rapidly as far as the extremities, where it forms two equal curves which are convex in relation to the magnet, and of which the equation has been given by Biot. It is the same on every line parallel to the axis, with this difference— that the ordinates of the curves are greater towards the edges than in the middle. I have studied only the axial curve; it is this which will be exclusively considered in what follows.

For one and the same steel magnetized to saturation, F augments with the thickness of the plate, according to laws (pro-

bably complicated) which I have not yet studied; it does not vary sensibly with the breadth. All my experiments have been made upon steel springs of 1 millim. thickness. The laws which I shall state will probably apply to other thicknesses, with different values of the coefficients.

I. When two similar magnetized plates are superposed, the curves representing the values of F rise, from the magnetism leaving the faces which are placed in contact to take refuge at the exterior parts. At the same time the curves approach each other and the middle of the magnet. This effect is increased on the addition of a third and a fourth plate. Finally the two curves unite in the middle.

From this moment the combination of force is at its maximum. A greater number of plates makes no change in its intensity at any point; and if we dismount the pile in order to study separately each of the layers which compose it, we find that they have lost a portion of their first magnetization as much greater as there were more of them. In short, all addition to the limit-number of the plates is to no purpose and a useless expenditure of steel. This final magnet, the only one susceptible of precise definition, is the only one to be employed, since it gives the maximum of effect: I shall call it the *normal* or *boundary magnet*. It will be seen that then all magnetic questions become unexpectedly simple.

II. The curve representing the force F in the normal magnet is a parabola represented by the equation $F = Ax^2$, x being the distance to the centre of the plate, and A a coefficient which varies with the length. This law is proved by the following numbers. It will be observed that the values of F rise rapidly at first with the number of layers, arriving very slowly at their maximum, which they do not afterwards exceed. (l denotes the half-length.)

Values of the force F.

$$2l = 480.$$

Distance to the centre.	3 plates.	7 plates.	9 plates.	15 plates.	Calculated.
millims.	grms.	grms.	grms.	grms.	grms.
240	41	42	48·2	54·3	57·6
220	25·4	36	40·2	45·0	48·4
190	13·9	25·6	32·6	27·2	36·1
140	9·5	17·0	16·5	20·1	19·6
90	......	7·8	8·5	8·6	8·1
40	......	1·2	1·5	1·5	1·6

$2l=310.$

Distance to the centre.	1 plate.	2 plates.	3 plates.	5 plates.	6 plates.	8 plates.	12 plates.	Calcu-lated.
millims.	grms.	grms.	grms.	grms.	grms.	grms.	grms.	grms.
155	12·5	22·1	28·0	35·5	37·0	38·4	37·8	35·4
145	8·5	15·0	20·7	30·7	30·2	30·5	34·0	32·3
125	4·0	7·6	16·7	21·2	24·3	23·5	25·0	24·0
105	1·5	4·5	10·2	15·0	18·0	18·5	18·0	16·9
85	0·6	2·0	7·5	10·0	12·7	15·6	14·2	10·8
55	......	0·6	2·5	5·0	7·1	7·0	7·6	4·5
35	......	......	1·4	3·0	3·5	3·2	3·0	1·4
15	......	......	0·2	0·5	0·5	0·7	1·5	0·3

$2l=250.$

Distance to the centre.	1 plate.	2 plates.	3 plates.	4 plates.	7 plates.	11 plates.	30 plates.	Calcu-lated.
millims.	grms.	grms.	grms.	grms.	grms.	grms.	grms.	grms.
125	14·5	24·6	28·2	28·6	31·6	27·6	29·6	29·6
105	10·0	12·7	16·2	18·7	18·2	19·7	19·9	20·9
75	3·0	7·0	8·6	11·2	11·2	11·2	11·2	10·4
25	......	1·0	1·5	1·4	1·4	1·4	1·4	1·2

$2l=200.$

Distance to the centre.	1 plate.	2 plates.	3 plates.	4 plates.	6 plates.	Calculated.
millims.	grms.	grms.	grms.	grms.	grms.	grms.
100	10·0	20·0	26·5	25·0	25·0	24·0
90	6·0	13·5	13·5	20·5	20·8	19·4
70	3·2	8·9	11·2	12·4	13·0	11·7
50	2·0	5·0	7·5	6·9	7·0	6·0
20	0·4	1·0	1·7	1·4	1·5	0·9

$2l=100.$

Distance to the centre.	1 plate.	2 plates.	3 plates.	5 plates.	Calculated.
millims.	grms.	grms.	grms.	grms.	grms.
50	7·6	9·7	11·4	12·7	12·0
40	4·7	7·0	7·7	7·8	7·6
30	3·4	5·5	5·5	5·5	4·3
20	1·5	2·5	2·0	1·8	1·06
10	...	1·5	0·4	0·4	0·4

III. It is difficult to say at what number of plates the maxi-mum is reached, since it is only slowly arrived at; but it is evident that the number is greater the greater the length. Three or four are required for 100 millims., six to eight for 200,

nine to fourteen for 300. We may say it is approximately proportional to the length.

IV. The preceding Tables show that the separating-force at the extremity of the normal pile augments with the length $2l$. It is 54 grammes for 480 millims., 38 for 310, 25 for 200, and 12 for 100. If we make the quotients of these forces by the half-lengths l, we find :—

Length $2l$.	480.	400.	310.	250.	200.	100.
F	54·0	44·1	37·8	31·5	25·0	12·5
$\dfrac{F}{l} = k^2$	0·225	0·220	0·244	0·252	0·250	0·250

The first two quotients are a little too low, because the numbers of the plates were not sufficient to give the exact limit of F; all the others are equal. We shall thence conclude that the separating-force F_l at the extremity of a normal pile is exactly proportional to its length—which is expressed by the formula $F_l = k^2 l$.

If l vary, $k^2 l$ will be represented by a right line Ac, making with the x-axis an angle of which the tangent is k^2,—k^2 varying doubtless with the thickness of the plates, but remaining constant when that thickness is invariable.

V. We have previously found that, for a given plate of length $2l$, F varies with the distance to the centre according to the law $F = Ax^2$. If $x = l$, we have $F_l = Al^2$, from which we deduce

$$F_l = Al^2 = k^2 l, \quad A = \frac{k^2}{l} ;$$

and consequently the general equation becomes

$$F = \frac{k^2}{l} x^2. \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

When the pile terminates in B, the parabolic curve of the values of F is A M C; if it is limited to D, it is A N E. All these parabolas are tangents at A to Ax, and pass through the points of the straight line A E C which correspond to the different lengths of the normal piles. In fine, the law of the separating-forces is expressed by means of a single coefficient k, which depends solely on the thickness of the elementary plate and on the kind of steel employed, the mean of k^2 being equal to 0·240. The values of F corresponding to the various plates, inserted in the preceding Tables, were calculated according to the above formula; sufficient accordance will be found between the observed and the calculated numbers.

VI. We shall assume, with Coulomb, that the separating-force

F is proportional to the magnetic intensity at each point, and that we can put $F = I^2$; then

$$I = \frac{k}{\sqrt{l}} x, \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

$$I_l = k \sqrt{l}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

Equation (3), which may be written $I_l^2 = k^2 l$, shows that the magnetic intensity at the extremity of the normal pile varies as the ordinates of a parabola A Q P tangent in A to the y-axis; and from equation (2) we gather that the intensity on the different points of a bar of length $2l$ is figured by a right line which makes with the x-axis an angle whose tangent is $\frac{k}{\sqrt{l}}$. For $l = A B$, this line is A P; it would be A Q for a pile terminated at the point D.
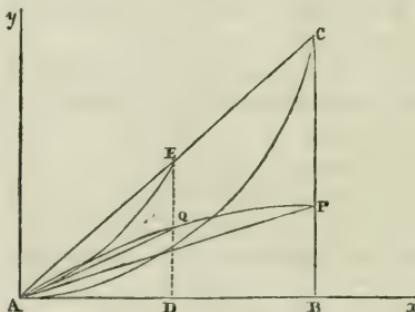
VII. This total M is the area of the triangle A B P, or

$$l \times k\sqrt{l} = kl^{\frac{3}{2}}.$$

If a be the width of the plates, and e their thickness, and if we neglect the augmentations of intensity produced at the corners and angles of the pile, this quantity must be multiplied by the perimeter $2(a+ne)$, n being the number of plates; we have therefore

$$M = 2(a + ne) kl^{\frac{3}{2}}.$$

VIII. When a contact is placed under the magnet, all free magnetism disappears if the contact is large enough and contains a sufficient quantity of iron. The whole of the magnetism M, therefore, is concentrated upon the surface of adhesion, which I name S. Its intensity there (that is, the quantity of magnetism on the unit of surface) is $\frac{M}{S}$; and the carrying-force will be $\frac{M^2}{S^2}$; for the whole surface it will be $\frac{M^2}{S^2} S$ or $\frac{M^2}{S}$.

It follows from this that the carrying-force is inversely as the surface of contact—which is correct, provided that all free magnetism has disappeared, but ceases to be true if S falls below certain proportions. It is on this account that cylindrical contacts are generally employed, and not plane ones. Substituting for M its value, the carrying-force P is

$$P = \frac{4(a + ne)^2 k^2 l^3}{S}.$$

IX. The weight of the magnet is equal to that of a plate, $2aeld$, multiplied by the number of plates, which is proportional to their length, and may be expressed by ml; it is therefore $\pi = 2mael^2d$. Consequently the ratio of P to π, which measures the carrying-force as a function of the weight of the magnet, will be

$$\frac{P}{\pi} = \frac{2(a + ne)^2}{Smaed} k^2 l,$$

or approximately, neglecting ne against a,

$$\frac{P}{\pi} = H \frac{al}{S} k^2.$$

This ratio will be proportional to the length and to the width of the plate, and inversely as the surface of contact.

X. There are two points which I have not examined in what precedes : they are the question of armatures, and the influence of the thickness of the plates. On the latter point I have ascertained the following :—

The power of a plate increases considerably with its thickness, but less rapidly than the thickness, so that there is a limit beyond which it remains stationary ; but a plate of thickness 1 has less power than two others of $\frac{1}{2}$ thickness, which are much less powerful than three plates the thickness of each of which is $\frac{1}{3}$; and, generally, the difference increases with the number of the layers of which a pile of a given thickness is composed. I have thus been induced to employ ribands of steel ; and as commerce supplies them abundantly and regularly, of excellent metal, I had only to superpose them in sufficient number to construct normal magnets and attain the extreme force, at the same time considerably diminishing the weight. I have thus obtained magnets carrying twenty times their own weight. I shall soon exceed this limit, thanks to the kind cooperation of M. Bréguet, and thanks also to my excellent and devoted foreman, Cyprien Bollé.

LV. *On the Law of Gaseous Pressure.* *By the* Hon. J. W. Strutt, *late Fellow of Trinity College, Cambridge.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

ABSENCE from England has prevented my seeing Mr. Moon's last paper (Phil. Mag. Feb. 1873) until lately. I question whether there would be any use in continuing the controversy, as we seem to have hardly any common ground for argument; but Mr. Moon asks me one or two definite questions which I ought not to leave unanswered.

I am asked whether I still "consider that Boyle's law has been experimentally proved in the case of motion." If forced to give a categorical answer, I must say yes; for the simple reason that all the experiments on gases ever made relate to the case of motion. Since my first note on this subject, Mr. Moon has explained, or at least admitted my assertion, that the absolute velocity of a gas is not material; so that in fact by motion he means *relative* motion of the various parts of a gas. I am free to admit that the experiments by which Boyle's law is established do not extend to the case of relative motion.

Mr. Moon finds a *reductio ad absurdum* of the received law of pressure in an argument relating to the behaviour of air confined under a piston when a weight is placed on the latter, and contends that the fallacy consists in the false assumption of the received law. I took some pains to point out that it is not merely the truth or falsehood of Boyle's law (as extended) which is at issue, but that Mr. Moon's argument, if valid at all, goes the length of proving that it is impossible, without self-contradiction, even to conceive a medium whose pressure shall (under all circumstances) vary as the density. My position is that the fallacy or absurdity lies not in the premises, but in the argument, which I applied to prove that a body would not fall when its support is removed. Mr. Moon does not admit the parallelism of the two paradoxes, and asks me to point out exactly where the fallacy lies. To enter into a complete explanation would be to write a dissertation on the principles of the differential calculus, for which this is certainly not the place; but I may say that (in my opinion) the error lies in the omission of the word *finite*. There can be no finite change of pressure without a finite displacement, nor can there be a finite displacement without a finite change of pressure. This much is admitted; but it does not follow that there is never a finite displacement or change of pressure. In fact the quantities *become finite*

together, or at least may do so, for all that this argument proves
to the contrary.

<div align="center">

I am, Gentlemen,

Your obedient Servant,

JOHN W. STRUTT.
</div>

May 15, 1873.

<div align="center">

LVI. *On some Improvements in Electromagnetic Induction Ma-
chines.* By H. WILDE, *Esq.**

[With a Plate.]
</div>

SOON after my announcement (in 1866) of the discovery that
electric currents and magnets indefinitely weak could, by
induction and transmutation, produce magnets and currents of
indefinite strength†, a number of electricians suggested other
methods by which this principle could be exhibited and more
powerful results obtained than those which I had described.

The most interesting as well as the most useful of these sug-
gestions was to augment the magnetic force of the elementary
magnet, by transmitting the direct current from the armature of
a magneto-electric or an electromagnetic machine through wires
surrounding its own permanent or electromagnet, in such a direc-
tion as to intensify its magnetism, until, by a series of actions
and reactions of the armature and the magnet on each other, an
exalted degree of magnetism in the iron or steel was obtained.

This idea seems to have occurred to several electro-mechani-
cians almost simultaneously in England, Germany, and America.
In a letter to the 'Engineer' newspaper of July 20, 1866, Mr.
Murray, after referring to my experiments, writes that he wishes
to point out a variety of the principles embodied in the machine
I had described, which, he says, is so obvious that it cannot fail
to be hit upon by some inventor before long, and warns any one
whom it may strike against patenting the idea, seeing that he
had already constructed a machine upon the plan. Mr. Mur-
ray then states that " whereas Mr. Wilde, beginning with an
ordinary magneto-electric machine, uses the current obtained
from it to charge a powerful electromagnet, and from this ob-
tains a second and more powerful current, which, used in like
manner, produces one still more intense, I, using only a single
machine, pass the currents from its armatures through wires
coiled round the permanent magnets in such a direction as to

---

intensify their magnetism, which, in its turn, reacts upon the armatures and intensifies the current."

Whether it be that electricians are not in the habit of reading engineering journals, or that information communicated to them in the form of letters is lost in the plethora of printed matter of all kinds which engages the attention of the reading public, Mr. Murray's warning to inventors against patenting his idea has been disregarded, as a patent was taken out on December the 24th of the same year by C. and S. A. Varley for "improvements in the means of generating electricity," wherein is described a machine consisting of two electromagnets and two bobbins. The bobbins are mounted on an axle, on which also a commutator is fixed; the ends of the insulated wire surrounding the bobbins are connected with this commutator, and through it with the insulated wire of the electromagnets, forming the whole into one electric circuit. Before using the apparatus, an electric current is sent through the electromagnet for the purpose of securing a small amount of permanent magnetism in the iron core of the electromagnet. On revolving the axle, the bobbins become slightly magnetized in their passage between the poles of the electro-permanent magnets, generating weak currents in the insulated wire surrounding them. The effect of the current passing through the electromagnets is to increase their magnetism and magnetize in a higher degree the bobbins when passing between the poles of the electromagnets; and in this way the electromagnets and the bobbins act and react on each other, causing the circulation of increased quantities of electricity.

Another patent for the same idea was taken out by C. W. Siemens, F.R.S., on January the 31st, 1867, as a communication from Dr. Werner Siemens, of Berlin. The invention is described as having for its object the obtaining of powerful electric currents without the aid either of large batteries or of permanent magnets, by the following method :—A movable keeper or armature surrounded with a coil of insulated wire is arranged in front of the poles of an electromagnet; and after rotatory motion is imparted to the armature, a magnetic impulse is given to the electromagnetic arrangement by the momentary insertion of a galvanic battery into the circuit, which steadily and rapidly augments simultaneously with an increasing electric current in the coils. For reproducing a current after the machine is arrested no fresh impulse from the battery is needed, because the residual or permanent magnetism of the electromagnet is sufficient to commence inductive action.

Although private letters addressed from one person to another ought never to be received as evidence in questions affecting the priority of scientific discovery or invention, yet, for the pur-

ARMATURES.

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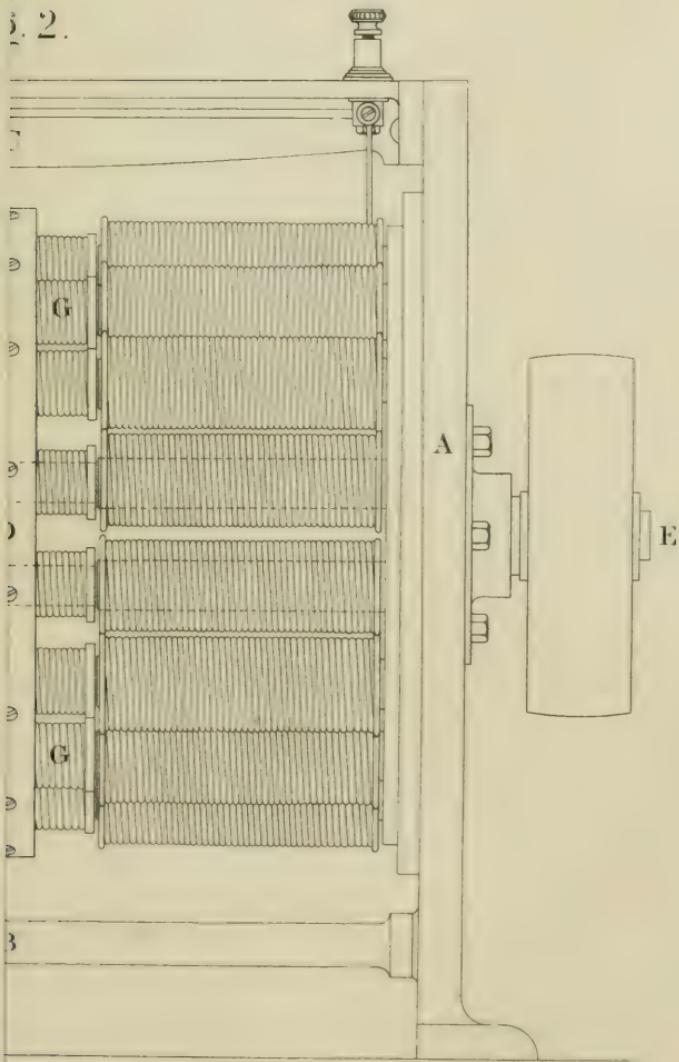
pose of showing the interest which attached to my investigations wherever science is cultivated, and also that when principles are once discovered the similar trains of reasoning of different persons lead to similar results, I will here mention that in the month of November 1866 I received a letter from Mr. Moses G. Farmer, of Salem, Mass., U.S.A., on the subject of my researches, and stating that he had built a small machine in which a current from a thermo-electric battery excites the electro-magnet of my machine to start it, and after the machine is in action a branch from the magneto-electric current passes through its own electromagnet, and this supplies the magnetism required.

The next and last instance of the repetition of this idea to which I shall refer (though my list is not yet exhausted), is that communicated to the Royal Society, February 14, 1867, by Sir Charles Wheatstone, in a paper " On the Augmentation of the Power of a Magnet by the reaction thereon of currents induced by the Magnet itself "*.   After pointing out that he had constructed the electromagnetic part of his machine according to my description, Wheatstone states that he first excites the electromagnet by any rheomotor, and, after removing it from the electromagnet, the circuits of the armature and electromagnet are joined to form a single circuit.   The electromagnet, retaining a slight residual magnetism, is therefore in the condition of a weak permanent magnet.   The motion of the armature occasions feeble currents in the coils thereof, which, after being rectified in the same direction by means of a rheotrope, pass into the coils of the electromagnet in such a manner as to increase the magnetism of the iron core ; the magnet having thus received an accession of strength, produces in its turn more energetic currents in the coil of the armature ; and these alternate actions continue until a maximum is obtained.

I have now enumerated, with some degree of tediousness and, to prevent misunderstanding, as nearly as possible as they have been described, instances where the idea of augmenting the force of a magnet by currents induced by itself has been repeatedly suggested.   This enumeration I should have deemed somewhat unnecessary were it not that a prominent worker in science, whose genius and attainments entitle his opinions to a high degree of respect, has described the contrivance (in a manner to produce in the minds of those interested in education an erroneous impression) as a new principle in electric science, a great step in magneto-electricity, and the discovery of Messrs. Siemens and Wheatstone†.

* Proceedings of the Royal Society, vol. xv. p. 369.
† Notes of a course of seven lectures on Electrical Phenomena and

That this contrivance suggested itself independently in different countries to the several experimenters above mentioned, there is as little reason to doubt as that the similar animistic ideas and customs found amongst the primitive races in various parts of the world are of independent origin ; but such repetitions exclude the contrivance from the rank of a discovery in science ; and it is, as Mr. Murray justly designates it, an obvious variety of the principles embodied in the machine I first described before the Royal Society.

At the time when this method of exciting an electromagnet was brought prominently forward by Messrs. Siemens and Wheatstone, I directed attention to the fact (which would seem to have escaped the notice of these electricians, as they omitted to mention it) that machines constructed as they had described them are incapable by themselves of producing powerful electric currents, as the whole energy of the machine is expended in exciting its own electromagnet*. Besides this, the actual amount of electricity circulating round the electromagnet is really very small ; and when the circuit is opened for the purpose of applying the current to some useful purpose, the magnet immediately discharges itself and resumes its neutral condition till the continuity of the metallic circuit is reestablished.

While the current transmitted from the armature of a magneto-electric or an electromagnetic machine, as I have said, is incapable of directly producing powerful electrodynamic effects, such current may be usefully employed to excite the electromagnets of other machines in accordance with my original method. Some idea of the smallness of the quantity of electricity requisite for this purpose will be formed from the fact, that the full power of the 10-inch machine is developed when its electromagnet is excited by the current from four pint Grove's cells.

This machine has been in constant operation for some time past for the electro-deposition of metals from their solutions ; and its electromagnet is now excited by its own residual magnetism in the following manner :—A small magnet-cylinder (3·5 inches diameter and 14 inches long) is bolted to the top of the 10-inch cylinder, so that the sides and axis of the former are parallel with the similar parts of the latter. The cylinders are separated for a space of three quarters of an inch by a packing of brass, and consequently act upon each other by induction through the intervening space, instead of by contact as in ordinary me-

Theories, delivered at the Royal Institution of Great Britain, 1870, by John Tyndall, LL.D., F.R.S., " desired by persons interested in education."
* Proceedings of the Literary and Philosophical Society of Manchester, vol. vi. p. 103.

thods of magnetization.   The
annexed figure, representing an
end view of the cylinders with
the armatures in section, will
make this arrangement of the
cylinders pretty clear to those
who are familiar with the con-
struction of these machines.

The residual or permanent
magnetism of the large electro-
magnet with its cylinder is very
considerable, being many times greater than that of the four
small permanent magnets with which it was originally excited*.
The coils of the small armature are placed in connexion with
those of the great electromagnet; and when the armature is ro-
tated, the magnet-cylinders act and react on each other until the
electromagnet is excited to the highest degree of intensity.   By
this arrangement of the armatures and cylinders, the minor cur-
rent for exciting the electromagnet is kept distinct from the
major current from the larger armature, which may be coiled
for currents of high or low tension according to the purpose for
which they are required.

It is essential for the attainment of a high degree of magne-
tism in an electromagnet excited by magneto-electricity, that the
continuity of the armature and electromagnetic circuits should
be preserved during the change of contacts from one segmental
part of the commutator to the other.   For this purpose the seg-
ments are made to overlap each other for a short distance, so
that the metallic rubbers or brushes for taking off the current
bear on adjoining segments simultaneously at the point of no
current, and, in so doing, form two closed metallic circuits for a
brief interval, which may be represented by the numeral 8, the
upper part of the figure representing the armature circuit, and
the lower part that on the electromagnet; but when the arma-
ture is at that part of its revolution when the current begins to
rise in intensity, the coils of the armature and electromagnet
form one continuous circuit, which may be represented by the
cipher 0.   The importance of keeping the circuits closed in the
manner described was not sufficiently observed in my earlier
experiments, and necessitated the employment of much more
powerful currents for exciting the electromagnets than were
afterwards found to be necessary.

* The small scale upon which my experiments have been repeated by
physicists has in some instances given rise to the idea that the residual
magnetism of an electromagnet is a lower degree of permanent magnetism
than that which originally formed the basis of my augmentations.
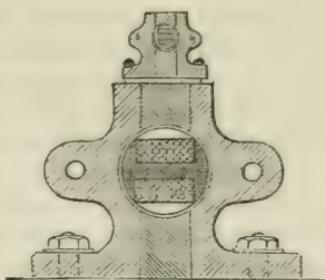
So far as I have communicated the results of my investigations on the principle of accumulative action in electrodynamics, they have been obtained with machines designed with reference to the peculiar form of armature contrived by Dr. Werner Siemens, of Berlin. While possessing several advantages in point of efficiency over that of Saxton, the Siemens armature requires to be driven at a high velocity to produce a succession of currents sufficiently rapid to be available as a substitute for the voltaic battery.   Little inconvenience, however, arises from the high speed when the armatures are of small dimensions; but as the dimensions increase it becomes necessary to lower the speed, and the large machines are consequently not proportionately powerful in comparison with the smaller ones.   Besides this, the advantage possessed by this form of armature, in having the moving mass of metal near the axis of rotation, is neutralized as the dimensions increase, by the excessive heat generated by the magnetization and demagnetization of the iron.   It would also be convenient in some circumstances to drive a machine direct from the crank or flywheel of a steam-engine without the intervention of multiplying gearing.

Considerations of this nature led me, towards the end of 1866, to propose to myself the construction of an electromagnetic machine with multiple armatures, which should remove the inconveniences inherent in those hitherto constructed by producing a greater number of currents for one revolution of the armature axis.   Since that time I have been engaged, with more or less interruption, in carrying out this design, and have at length constructed a machine the performance of which surpasses all my previous essays in this direction in regard to power and efficiency, and with a considerable reduction in the quantity of the materials employed*.

The machine in which these results are embodied is represented in Plate VIII. figs. 1 and 2.   In these views A_{ll}, A_{l} are the two sides of a circular framing of cast iron, firmly fixed together by the stay rods B_{ll}, B_{l} and the bridge C_{l}.   A heavy disk, D_{ll}, of cast iron is mounted on a driving shaft E_{ll}, running in bearings fitted to each side of the framing.   One of these bearings, F_{ll}, is carefully insulated from the framing by suitably formed pieces of ebonite, and also from the shaft by a cylinder of the same substance.   Through the side of the disk and parallel with its axis sixteen holes are bored, at equal angular distances from

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* To afford myself leisure to carry out my designs on a large scale without being involved in questions affecting the priority of my results, a general description of the improvements which form the subject of this paper was deposited in due form with the Commissioners of Patents, London, December 1866 and March 1867, and with the Ministère de l'Agriculture et des Travaux publics, Paris, June 1867.

each other, for the reception of the same number of cores or armatures, G_r. The cores project about two inches through each side of the disk, and are held firmly in their places by screws tapped through its periphery. Around each inside face of the circular framing, and concentric with the driving-shaft, sixteen cylindrical electromagnets are fixed at the same angular distance from each other and from the centre of the shaft as the iron cores round the disk; the two circles of magnets consequently have their poles opposite each other, with the disk and its circle of iron cores revolving between them. The ends of the cores are terminated with iron plates of a circular form, which answer the double purpose of retaining the helices surrounding the cores in their places, and overlapping for a short distance the spaces between the poles of the electromagnets. The closing of the *magnetic* circuits of the electromagnets and armatures for a short distance, like the closing of the *electric* circuits for a brief interval at the point of no current, has a marked influence on the power of an electromagnetic induction machine,—both contrivances conspiring, simultaneously, to maintain the magnetic intensity of the electromagnets during the rise and fall of the magneto-electric waves transmitted through the helices.

The cylindrical bar magnets are each coiled with 659 feet of copper wire 0·075 of an inch in diameter, insulated with cotton ; the helices are grouped together to form a fourfold circuit 2636 feet in length, and are joined up in such a manner that adjacent magnets in each circle, as well as those directly opposite in both circles, have north and south polarity in relation to each other. A charge of permanent magnetism was imparted to the system of electromagnets by the current from a separate electromagnetic machine.

The armatures, although formed of sixteen pieces of iron, are, by projecting through both sides of the disk, thirty-two in number. The length of insulated wire on each armature is 116 feet ; and the thickness is the same as that on the electromagnets. These helices are divided into eight groups of four each, and coupled up for an intensity of 4×464 feet. One of the groups is used for producing the minor current for exciting the circle of electromagnets, while the remaining groups are joined together for a quantity of seven, and an intensity of four for the production of the major current from the machine. The aggregate weight of wire on the electromagnets is 356 lbs., and on the armatures 26 lbs. The helices for exciting the electromagnets are connected with the commutator H, while those producing the major current are placed in connexion with the rings I, K, or in place thereof with another commutator, according as the alternating or the direct current from the machine is required.

The strength and proportions of the several parts of the machine enable it to be driven with advantage from 300 to 1000 revolutions per minute.

At the medium velocity of 500 revolutions per minute the major current will melt eight feet of iron wire 0·065 of an inch in diameter (No. 16, B. W. G.), and will produce two electric lights in series, each consuming carbons half an inch square at the rate of three inches per hour.

When driven at a velocity of 1000 revolutions (equivalent to 16,000 waves) per minute, the current will fuse twelve feet of iron wire 0·075 of an inch in diameter (No. 15, B. W. G.). As soon as the heating or fusing of an iron wire of a given length and section comes to be an acknowledged measure of powerful electric currents, as well as a method of comparison between the power of electromotors of different kinds, as it must ultimately be, the significance of this result will be fully realized.

At the velocity of 1000 revolutions per minute the light from two sets of carbons in series is unendurably intense, as well as painful to those exposed to its immediate influence. Estimated on the basis afforded by the performance of the excellent magneto-electric-light machines of MM. Auguste Berlioz and Van Malderen, who have made a careful study of the photometric intensity of the electric and oil lights, the power of the new machine is equal to that of 1200 Carcel lamps, each burning 40 grammes (1·408 oz. avoird.) of oil per hour, or of 9600 wax candles. The amount of mechanical energy expended in producing this light is about 10 indicated horse-power.

A comparison between the power of the new machine and that of the 10-inch machine will show that while the current from the former fuses twelve feet of iron wire 0·075 of an inch in diameter, the current from the latter fuses only seven feet of wire 0·065 of an inch in diameter, and is consequently only about half as powerful as that from the new machine. Besides this, the quantity of copper used in the construction of the new machine is about $3\frac{1}{2}$ hundredweight, and of iron 15 hundredweight, while the weight of these metals in the 10-inch machine is 29 and 60 hundredweight respectively. In other words, we have in the new machine a double amount of power with less than one fourth the amount of materials employed in the construction of the 10-inch machine.

Another advantage possessed by the new machine is the great reduction of temperature in the armatures by their rapid motion through the air, which acts much more efficiently than the circulation of water through the magnet-cylinder. By increasing the diameter of the electromagnetic circles conjointly with the number of electromagnets and armatures, the angular velocity of the

machine may be so diminished that it may be driven directly
from the crank of a steam-engine, concurrently with an increase
of electric power proportionate to the number of electromagnets
and armatures in the electromagnetic circles.

While the excitation of the electromagnets of the machine by
the current from several of its armatures is attended with some
advantages where portability is required, yet, as provision has to
be made for keeping the major and minor currents separate from
each other, the commutator arrangements become somewhat
complicated, and faults in either of the circuits are not so readily
localized as when a separate exciting machine is employed. In
those cases, therefore, where conveniences for driving separate
machines are at hand, and when the power of several of them is
required simultaneously, as in large electro-depositing establish-
ments, some advantage will be gained by using a separate ma-
chine of suitable power to excite the electromagnets of several
machines, when the currents from the whole of them may be
utilized, and a commutator on the axis of each machine will be
dispensed with.

In my paper "On a Property of the Magneto-electric Current
to control and render Synchronous the Rotations of the Armatures
of a number of Electromagnetic Induction Machines"*, I stated
that this property would be available when the machines were
used for the electro-deposition of metals from their solutions.
It has, however, been found that the small resistance presented
by depositing solutions to the passage of the currents prevents
this property from manifesting itself (in accordance with what I
stated in my paper respecting the effect of joining the poles with
a good conductor) ; and it is only when the machines are em-
ployed for the production of electric light or other purpose
where the external resistance is considerable, that this electro-
mechanical function of the current comes into useful operation.

Before concluding my description of this further development
of the principle of electromagnetic accumulation, I consider it
a duty I owe to myself as well as to science that I should not
allow to pass unnoticed the views and statements of certain
writers respecting the place and value of my investigations in
the history of natural knowledge. The peculiar good fortune
which enabled me to follow up the discovery of a great principle
to such brilliant results has contributed, accidentally in some
instances, to establish the idea that these results are an expan-
sion of Faraday's discovery of magneto-electricity rather than a
distinct step in electrical science. A brief glance at the history
and progress of electricity and magnetism will suffice to show

* Proceedings of the Literary and Philosophical Society of Manchester,
December 15, 1868. Phil. Mag. S. 4. vol. xxxvii. p. 54.

the erroneousness of this view, and also that my discovery bears only the same kind of relation to that of Faraday as that philosopher's discovery does to those of Galvani, Volta, and Grove in galvanic electricity, and of Œrsted, Ampère, Arago, and Sturgeon in electromagnetism. That the discovery of the indefinite increase of the magnetic and electric forces from quantities indefinitely small is a fundamental advance in electrical knowledge, and not simply an expansion of known principles, or an improvement in a machine (as it has been made to appear by some), is evident from the fact that the principle, since its enunciation in 1866, together with my invention of minor and major magneto-electric circuits, has been embodied in the machines of different forms constructed by Ladd, Holmes, D'Ivernois, Gramme*, and

* *Comptes Rendus de l'Acad. des Sci.* July 1871, December 1872. A novel feature in the machine constructed by M. Gramme is an attempt to arrive at a nearer approximation to the continuous current of the voltaic battery than that produced from a magneto-electric machine when rectified by means of a commutator of the ordinary construction. This refinement, however, possesses little or no advantage in any of the applications of magneto-electricity when the rectified waves succeed each other at the rate of 5000 per minute and upwards—a rate of succession easily attainable, and far exceeded by the machines of Berlioz and Holmes. At this rate the discontinuity of the waves is not distinguishable in the electric light, nor in the magnetization of electromagnets, nor on galvanometer needles, nor in electrolytic processes; and it can only be perceived by the vibrations of a steel spring placed before the poles of a small electromagnet round which the current is transmitted. Such instrument would, I think, also indicate similar points of maxima and minima in the current from Gramme's machine. As the armature-helices in this machine are each connected with separate pieces of metal, forming the segments of a circle, from which the current is taken by means of ordinary metallic brushes, the number of helices producing currents available for external use at any given moment is only a fraction of those constituting the whole circle; and consequently for a given weight of materials such a magneto-electric machine must be greatly inferior in power to machines in which the current is delivered from the whole of the helices simultaneously, as in those hitherto constructed. The substitution, by M. Gramme, of a commutator with multiple segments insulated from each other and having adjacent segments of the same polarity, while those diametrically opposite have a polarity different, requires the same precautions to be taken to prevent the spark at the change of contacts, and is subject to the same wear from friction as commutators of the ordinary form, in which the segments are united with a common metallic base. Moreover, long experience has proved that, for the production of electric light, the alternating current is greatly superior to the continuous one—as commutators are dispensed with, and it has the important advantage of consuming the carbons equally, and thereby always retains the luminous point in the focus of any optical apparatus used in connexion with it.

In short, M. Gramme, in his endeavour to reconcile the incompatible relations of the voltaic current and the magneto-electric wave at the instant of its generation, has, by inverting the order and functions of the organic parts of an ordinary magneto-electric machine and suppressing the action of a number of the armature-helices, brought about results retro-

others. Moreover Faraday himself, while on the threshold of
my discovery, distinctly negatived its possibility. Reasoning on
the magnet as a source of electricity, in a paper " On the Physical
Character of the Lines of Magnetic Force " (Phil. Mag. S. 4.
vol. iii. p. 415), he says, " Its analogy with the helix is won-
derful; nevertheless there is as yet a striking experimental di-
stinction between them; for whereas an unchangeable magnet
can never raise up a piece of soft iron to a state more than equal
to its own, as measured by the moving wire, a helix carrying a
current can develope in an iron core magnetic lines of force of a
hundred or more times as much power as that possessed by itself
when measured by the same means. In every point of view,
therefore, the magnet deserves the utmost exertions of the phi-
losopher for the development of its nature, both as a magnet
and also as a source of electricity, that we may become acquainted
with the great law under which the apparent anomaly may dis-
appear, and by which all these various phenomena presented to
us shall become *one.*" Now it was the precise and absolute
manner in which Faraday stated the definiteness of the relation
between the magnetism of a permanent magnet and that of a
piece of iron magnetized by its influence, that led me to enun-
ciate, in terms equally absolute and precise, the antithesis of
his proposition. The anomalous difference between the mag-
netic properties of a helix and an iron core to which Faraday
directed attention is explained by the property possessed by
an iron core, when surrounded with a helix of great length,
of acquiring and retaining for a sensible time the magnetic
charge in a manner analogous to that by which the Leyden jar
acquires a charge of electricity—the acquisition of the charge
in the former, as in the latter, being the more rapid as the
power of the electromotor is increased. How far Faraday's
hopes and preconceptions of the electromagnet as a source of
electricity have been realized the results described in this and
my former papers will show. Already has it superseded the
use of the voltaic battery in every electro-depositing establish-
ment of note in this country, and it is making rapid progress
abroad.

That the transformation of mechanical energy into other
modes of force on so large a scale and by means so simple will
find new and much more important applications than that above

gressive from those previously obtained by Nollet, Berlioz, and Holmes;
and it is only by the adoption of the principle of electrodynamic accumu-
lation (*i. e.* the exciting of a major electromagnetic induction machine by
a minor one fixed on the same base), in accordance with the principles laid
down in my former papers, that the results obtained by M. Gramme exceed
those from ordinary magneto-electric machines.

mentioned is one of my most firm convictions. Even now several of such applications begin to foreshadow themselves, by which the electromagnet, as a source of electricity, is destined hereafter to live in the lives of the millions of mankind when the memory of its origin, except with the curious and the learned, shall be forgotten.

LVII. *The first Extension of the term* Area *to the case of an Autotomic Plane Circuit.* By Thomas Muir, *M.A., Assistant to the Professor of Mathematics in Glasgow University*[*].

IN this country mathematicians know of the use of the word *area* as applied to an autotomic plane circuit through De Morgan's paper " On the Extension of the word Area " in the ' Cambridge and Dublin Mathematical Journal' for 1850, and a paper consequent upon this by Sir William Thomson, " On the Potential of a Closed Galvanic Circuit of any Form." The use may be shortly yet accurately explained thus :—The course followed in tracing the circuit is, first of all, carefully marked by means of arrow-heads ; the area (in the ordinary sense) of each cell is then taken a certain number of times, viz. the number of times the circuit is crossed from right to left diminished by the number of times it is crossed from left to right in coming in the plane from a point wholly outside the circuit to any point within the cell in question ; and the sum of these products is the area of the circuit.

This extended meaning being invaluable in the generalization of geometrical theorems, the question of its first publication becomes a matter of some importance in the history of mathematics. De Morgan probably considered himself pioneer in the matter ; he says, in the publication above cited, " no such extension of the word has been made that I ever met with." The object of the present notice is to direct attention in correction of this to a paper by Alb. Ludov. Fried. Meister[†] in the ' Göttingen Commentaries' for 1769–70, entitled " De Genesi Figurarum Planarum et inde pendentibus earum Affectionibus," which contains almost all that even yet can be said on the subject.

The paper, which is well arranged and bears marks of careful preparation, extends to thirty-seven pages quarto, and is illustrated by nine large plates containing in all about fifty separate figures. After a preface of four pages and a short introductory

paragraph, the author enters upon the first section of his subject, viz. :—

Descriptio figuræ per motum rectæ parallelum.

At the outset two conventions (or *lemmas*, as they are called) are adopted, which may be paraphrased as follows :—

1. A straight line, whether positive or negative, having parallel motion and moving first in one direction and then in the opposite, thereby describes parts of the same figure which are affected with opposite signs.

2. A straight line which, during motion in one direction, is at one time positive and at another negative, thereby describes parts of the same figure which are affected with opposite signs.

A circuit resembling the lemniscate in the character of its area is then considered. The describing line begins at *g* (fig. 1), where it has no magnitude, moves parallel to itself from *g* to *c*, varying the while in length, changes sign in passing through *c*, and, then proceeding as before, at last vanishes and ceases to move at *f*. The area of the figure *g a c e f d c b g* is therefore, in accordance with the second convention, to be reckoned as the sum of two areas, *g a c b g* and *c e f d c*, affected with opposite signs.

As one might without special notice consider the point *c* in this figure either as a mere meeting-point of two portions of the perimeter, or as a point of actual intersection, the resultant area being

Fig. 1.

of course different in the two cases, the author is here led to remark on the importance of indicating in the diagram the mode of tracing the circuit. This, he says, can best be done by traversing the perimeter and bordering it on the right with one colour and on the left with another; so that at points where the opposite vertical angles have the same colour, the perimeter merely meets itself; and where they have different colours, actual intersection takes place. The colours also render important help in determining the sign which affects any particular portion of the figure. On this principle the author's diagrams are drawn; but instead of two colours, one on the
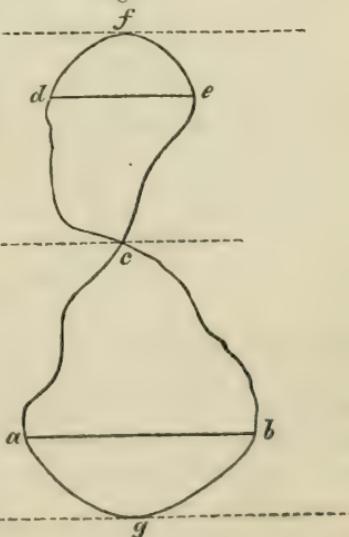
right and the other on the left, he uses a narrow border of sha-ding lines drawn at right angles to the circuit, and always on one and the same side throughout the course (see figs. 2, 3).

The next diagram being taken to illustrate the application of the first convention may be passed over, as both conventions are used in the consideration of the third diagram, to which we shall now refer. $a, d\, o, \epsilon\, n, m\, i, \kappa$ (fig. 2) are successive positions

Fig. 2.
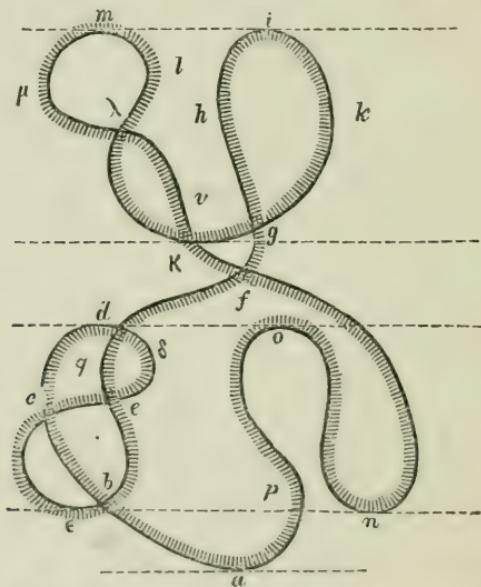
which the describing line occupies in the course of its parallel motion, a being the point where it comes into existence, and κ the point at which it ceases to exist. With this explanation and the guidance of the conventions, any reader may easily verify the result obtained for the area, which is " $+ ghikg + \kappa\lambda\nu\kappa* + ce\delta qdc + abeqfnopa$, so that the part $eq\delta e$ is reckoned twice, and $-lm\mu\lambda l - \kappa gf\kappa* - becb$." Moreover, as it is here observed that some portions of area external to the perimeter are *swept in* by the moving line only to be *swept out* by it later in its course, there arises the theorem—" The area which a straight line vary-ing in length from zero to zero describes in moving parallel to itself is equal to that of the figure whose perimeter is described by the extremities of the straight line."

In the next place is considered the case where the describing straight line during its motion never vanishes (and therefore never changes sign), and finally returns to the same position

* Misprints occur here in the original through a confusion of the Greek κ and the Italic k.

and magnitude as at starting. Here, of course, the extremities of the line trace out independent circuits, the algebraic sum of whose areas is equal to the area described by the line. The consideration of a particular case of this, viz. that in which the area of one of the circuits is zero, leads to a rule for finding the area of any given circuit; but this rule, though considered rather elegant (" *concinniorem methodum* ") by its author and therefore deserving of systematic proof, is cumbrous and troublesome as compared with De Morgan's. On this account it is not reproduced here.

The next section of the subject is

Descriptio figurarum per motum rectæ circularem.

The introduction starts with the theorem, " Any triangle is equal to the algebraic sum of the triangles whose common vertex is any point in the plane of the given triangle and whose bases are the given sides." This leads to the more general proposition, "The area of any plane figure whatever (including, of course, those whose perimeters are autotomic) is equal to the algebraic sum of the triangles having a common vertex in the plane of the figure and so constructed that their bases make up the given perimeter;" it being pointed out that in the case of figures whose perimeter is curvilineal, the simplest set of triangles is obtained by drawing from the given point in the plane all possible tangents to the perimeter. And now thus prepared we are invited to consider in succession the various cases of the description of figures by a straight line revolving now in one way now in another round a fixed point situated in the direction of its own length. These cases are six in number, and may, for the sake of condensation, be logically arranged as follows, the author's elucidations being omitted as superfluous:—

A. When the describing line preserves the same sign,

$\begin{cases} a, \text{ and is constant in length.} \\ b, \text{ and is variable in length.} \end{cases}$

$(a)\ (b) \begin{cases} 1, \text{ centre in one extremity of line.} \\ 2, \text{ centre between extremities.} \\ 3, \text{ centre in line produced.} \end{cases}$

It may be mentioned that many of the theorems in this section are stated alternatively in the language of projections, much circumlocution being thus obviated; *e. g.* the result of (A, *a*, 3) is put thus:—" In whatever plane a pyramid, however truncated, may be projected, the projection of the bases equals the projection of the sides."

Lastly, we have

Descriptio figurarum per motum rectæ quemcunque.

This, though more interesting and requiring more elucidation than the others, is treated very briefly. The motion considered is that compounded of the motions of the two preceding sections. The describing line is now subject simultaneously to translation and rotation; that is, it rotates round a variable centre. In certain of the cases as many as *three* circuits must be attended to, the third (not hitherto mentioned) being the locus of the centre of rotation. As an instance of this the author's thirteenth figure is given (see fig. 3).

Fig. 3.
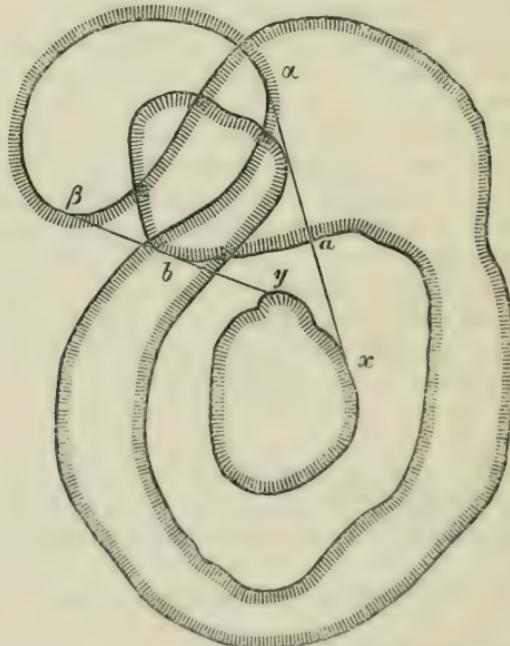
The remainder of the paper, comprising seven sections, does not treat de *genesi* figurarum, and need not, therefore, be referred to unless in the most cursory manner. It may be said to deal in the first place with COGNATE SUBJECTS, *e. g.* the *angles* of rectilineal figures having autotomic perimeters, *multiple circles* (*i. e.* circles of N × 360 degrees) and the figures inscribed in them, *stellate* regular polygons (*i. e.* regular polygons with autotomic perimeters), and in the second place with APPLICATIONS of the new ideas to the extension of known geometrical theorems.

LVIII. *Note on a Crystallized Compound of Sesquioxide of Iron and Lime. By* John Percy, *M.D., F.R.S., Lecturer on Metallurgy at the Royal School of Mines, London*[*].

IN 1861 I published the following statement [†]:—"*Sesquioxide of iron and lime.* I find that a mixture of these substances in certain proportions yields a well-melted slag. A mixture consisting of 160 grains of pure sesquioxide of iron and 100 of white marble (= 56 grains of lime)—that is, in the ratio of $Fe^2 O^3 : CaO$—was exposed in a covered clay crucible to a high temperature. It was perfectly melted, and when broken across resembled a black, opaque, vitreous slag: the crucible had one large perforation. In a second experiment a mixture, according to the same formula, of 40 grains of sesquioxide of iron and 25 of carbonate of lime was heated in a clay crucible lined with platinum-foil. It was perfectly melted, and escaped through the crucible." As in these experiments the heating of the crucible was effected in an air-furnace, and as the product was not analyzed, it was conceived that some of the sesquioxide of iron might have been reduced to protoxide, with the formation of a proportionate quantity of magnetic oxide, which, it is well known, is fusible at a high temperature; for H. Rose found that when sesquioxide of iron was heated *per se* in a porcelain kiln, it was reduced to magnetic oxide, which melted. In order to settle the point, the following experiments have been made. An intimate mixture of 190 grains of sesquioxide of iron and 66·5 grains of lime (*i. e.* in the ratio above mentioned) was kept heated to whiteness in a platinum vessel during several hours in a muffle, the atmosphere in which is oxidizing, and left to cool in the furnace during the night. The product appeared to have been perfectly melted, and consisted of a mass of interlacing acicular crystals exceeding an inch in length; it had a dark bright metallic lustre; it was very brittle, and when in the state of fine powder resembled brown iron ore in colour; its fracture was uneven and lustrous; its specific gravity was 4·693; and, what is interesting, it was magnetic.

Its composition *per cent.* was as follows :—

Sesquioxide of iron	73·39
Protoxide of iron	0·72
Lime	24·50
Silica	1·35
Alumina	0·10
	100·06

The existence of a little protoxide of iron is accounted for by the presence of silica, which at a high temperature, even in an oxidizing atmosphere, reduces sesquioxide of iron to protoxide, with the formation of silicate of this oxide—though, when this silicate is heated in an oxidizing atmosphere at a lower temperature, its protoxide of iron is converted into sesquioxide, the product after such treatment being merely a mechanical mixture of silica and sesquioxide of iron, as has been demonstrated by experiment.

Assuming the existence of a compound represented by the formula $CaO, Fe^2 O^3$, the calculated percentage composition is

Sesquioxide of iron . .	74·07
Lime	25·93
	100·00

A comparatively large quantity of this beautiful substance has recently been prepared by heating a mixture of 1600 grains of hæmatite and 1000 grains of chalk ($=560$ grains of lime) in a platinum vessel in a muffle, precisely as in the last experiment, except that at first the temperature was carefully moderated until the carbonic acid had been expelled from the chalk and the mixture had become soft. The mixture was kept at a white heat during about three hours, and left to cool gradually in the furnace during the night as the fire died out. The product weighed 2148 grains; but supposing no loss to have occurred and the materials to have been pure, it should have weighed 2160 grains. It had the same characters as that previously described; and some of the crystals on the surface were 2 inches long.

On a future occasion I hope to be able to communicate information concerning the crystalline system of this compound on the authority of a trustworthy crystallographer, and to report the results of analogous experiments with other oxides of the lime and sesquioxide of iron types respectively. I have much pleasure in stating that the foregoing recent experiments have been conducted, in the metallurgical laboratory of the Royal School of Mines, by my colleague, Mr. Richard Smith.

In chemical constitution this compound may be regarded as a lime magnetite, or magnetic oxide of iron in which the protoxide of iron is replaced by lime.

It should be stated that Lampadius more than seventy years ago subjected various mixtures of sesquioxide of iron and lime to high temperatures*; but as his experiments were made either in clay crucibles, or in similar crucibles lined with carbon, the results are obviously unsatisfactory.

* *Journal des Mines*, vol. xviii. p. 168.

LIX. *Notices respecting New Books.*

The Strength of Materials and Structures. By JOHN ANDERSON, C.E., LL.D., F.R.S.E., &c. London: Longmans, Green, and Co. 1872 (pp. 301).

THIS book consists of two parts, the first treating of "the strength of materials, as depending on their quality, and as ascertained by testing-apparatus," the second treating of "the strength of structures, as depending on the form and arrangement of their parts, and on the materials of which they are constructed." In the first part, after a few preliminary remarks, the author gives an account of the machine in use at Woolwich for testing the strength of materials, particularly when exposed to tensile or compressive stresses. He then passes in review the principal materials used in constructions, with especial reference to their behaviour in the testing-machine ; he devotes a chapter apiece to cast iron, wrought iron, and steel, one to copper and other metals and their alloys, one to timber, and finally gives, somewhat briefly, notices of the resistances offered by materials to impact, torsion, and shearing. The results obtained by experiment are gone into with a good deal of minuteness, and are also recorded in Tables, which, in the first part, are as many as thirty-eight in number. Many of these Tables give results which have, to all appearance, been obtained under the author's superintendence ; others are taken from writings of authority, *e. g.* from the Report of the Commissioners appointed to inquire into the Application of Iron to Railway Structures.

This part of the work contains a great deal of valuable information, and will repay attentive study by any one interested in mechanical science, whether a workman or not. A critical reader might notice here and there a trace of want of practice in literary composition ; this, however, is a trifling defect, and in nowise diminishes the solid value of a book written by one who has had a long and intimate acquaintance with his subject.

The second part is of a somewhat miscellaneous character : it is devoted to questions regarding the strength of girders, trusses, boilers, &c. Its chief interest consists in the fact that the structures which come under discussion have been actually erected by the author. The reader is therefore enabled to see how the proper mechanical rules are employed in designing actual works. To our mind, the best example given in this part of the work is that of the 30-ton crane, which forms a most instructive mechanical "study."

There is a tendency observable throughout the book to treat the subject as if it consisted of isolated points ; even where points are closely connected, the fact of the connexion is sometimes ignored. A good instance of this is the manner in which the author treats the subject of deflection. He lays down several rules applicable to the subject—for instance, that a rectangular beam, supported at both ends and loaded at the centre, has a deflection in

inches equal to

$$\frac{\text{cube of length in feet} \times \text{load in lbs.}}{\text{breadth in inches} \times \text{cube of depth in inches} \times \text{constant}}.$$

The Table registers the "constant" needed for reducing this formula to numbers for different materials. Now this constant is merely Young's "modulus" in disguise; *e. g.*, in the case of wrought iron the registered "constant" is 64,221, which is equivalent to a "modulus" of nearly 28,000,000 lbs. per square inch. There are several objections to this way of treating the deflection of beams; the chief is that it leads the learner to regard deflection as a subject by itself, whereas it is closely connected with simple extension and compression, produced by moderate forces. A reader who had not obtained the knowledge elsewhere would not suppose that there was any connexion between the constant just mentioned as given in Table 39, and the experimental results registered in the early part of Table 8.

There is another point which may be noticed, and which will be best introduced by extracting a part of p. 209. The figure "is," says the author, "a skeleton diagram of the form of trussed beam used for overhead travelling cranes. . . . . To ascertain the strain upon
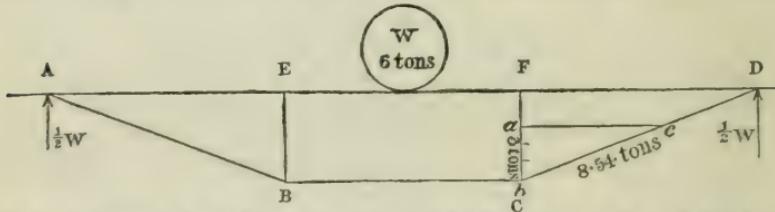
the top beam of such a structure, which is generally of timber, find the weight acting at the centre; multiply that weight by half the span of the truss, and divide by the depth of the truss. . . . . Let the beam be 20 feet span, and required to carry 6 tons at the centre; the depth of the truss is usually made $\frac{1}{8}$ of the span, and in this example is 2 ft. 6 in. The strain along the top bar will be $10 \times 6 \div 2\frac{1}{2} = 24$ tons. The compressive strength of timber may be taken at 6000 lbs. per square inch, and the safe working stress at $\frac{1}{10}$ of that amount, namely 600 lbs., and the sectional area of the material will be $24 \times 2240 \times 600 = 89\frac{3}{5}$ sq. in." The author adds that the stress on C D is 8·54 tons. We have no objection to make to the conclusion that about 90 square inches is a proper section for the beam for practical purposes; nor will we do more than notice that it is a little hard on the reader to expect him to find out for himself the reason of the rule by which the result is obtained. But we think the author ought to have noticed, and perhaps to have insisted on the point, that it is highly improbable that the stresses will have in reality the values assigned to them. To show this we will take a rather extreme case, and suppose that the beam is 3 in. wide and 30 in. deep, that A B and C D have nuts at the end by which they can be

slightly lengthened and shortened, and that the weight of the truss can be neglected. If the beam were not trussed, the deflection at the middle point would be, according to the timber employed, between a half and a quarter of an inch, and at E and F slightly less. If, then, we suppose A B and C D lengthened sufficiently to allow of the points E and F drooping to this extent, the compressive stress along A D instead of being 24 tons, will be exactly nothing, except so far as the upper part of the beam is compressed in the act of bending. If, now, A B and C D are gradually shortened, the component members of the truss will be brought into a state of stress, the amount of the stress being determined by the degree in which the droop at E and F is reduced. At each turn of the nuts the stresses will be varied; and when the operation is completed, the amounts of these stresses will in themselves be determinate, but will depend upon the extent to which the process of screwing up has been carried. If two such trusses were placed side by side, under apparently similar circumstances, it is quite possible that their actual states of stress might be materially different.

In spite of these defects, and some others which our limits will not allow us to notice, the book contains very much that is valuable, and we can cordially recommend it to the notice of students of mechanical science.

British Rainfall, 1872. *Compiled by* G. J. SYMONS. London: E. Stanford, Charing Cross.

The well-earned character of this very useful annual volume is fully maintained in the present issue, and not only so, but it contains three features of unusual interest—an important article on the measurement of snow, an examination of the alleged periodicity of rainfall synchronous with the period of sun-spot frequency, and a comparison of rainfall in 1872 with the three years of great rainfall, 1848, 1852, and 1860. In the article on the periodicity of rainfall, Mr. Symons enters at some length into the researches of Messrs. Meldrum and Lockyer, and quotes an elaborate paper by Dr. Jelinek to the effect that taking a broad view of the subject, the evidence is conflicting—a result that Mr. Symons had previously arrived at. Dr. Jelinek urges the importance of obtaining still further materials for estimating the connexion between sunspot frequency and depth of rain, and advises that the consideration of the data should be proceeded with quietly and without prejudice. At the close of the article Mr. Symons gives a list of some links in the chain which binds us to our central source of light and heat, in which we find "auroræ accordant with sun-spot prevalence." This reminds us that there is another link not mentioned by Mr. Symons, viz. the diurnal inequality of magnetic declination. Professor Loomis has furnished the 'American Journal of Science and Arts' with an interesting paper on the synchronous variations of the three phenomena—sun-spot frequency, mean diurnal range of magnetic declination, and auroral displays, from 1776 to 1872, illustrated with the curves of each. From the third feature above

mentioned in 'British Rainfall,' we gather that in 1872 the greatest quantities for that year, as compared with the other three, fell at fifty-six stations out of eighty-two, of which forty-five gave the maxima for the years 1848 to 1872. In the year 1852 sixteen stations furnished the greatest falls, fifteen being maxima from 1848 to 1872. In 1848 the largest falls were at eight stations, five being maxima as above. In 1860 there were only two stations at which the falls were the greatest for the four years named; there were no maxima at any of the eighty-two stations in 1860. Mr. Symons gives a supplementary Table of seventeen stations, at each of which the maximum rainfall occurred on other years than the four wet years discussed. The figures quoted from, and the quantities recorded in the Table of comparison mentioned show that in the majority of cases 1872 was *generally* the wettest year on record.

Weekly Weather Reports issued by the Meteorological Office.

We are glad to find that the Meteorological Office has resumed the issue of Weekly Summaries of the Weather in Western Europe. It is intended to accompany the chart issued on Wednesdays by a summary of the weather during the previous week, so that each day's map will be more readily intelligible when examined by the light thrown upon it by the summary, the course of a depression more easily traced, and the general character of the weather much better understood than it can be from the mass of figures comprised in the daily reports. There is, however, one feature which we are desirous should be introduced; it is a publication of monthly barometric curves at selected pairs of stations some distance apart from each other, as, for example, Haparanda and Biarritz, Christiansund and Scilly, Cuxhaven and Valencia, Toulon and Stornoway. If each pair of curves were projected on the *same* line of abscissæ, the ordinates would show at a glance the direction and amount of the gradients for the period of projection, say, one month. We have no doubt that one or two pairs of curves introduced monthly would greatly facilitate certain meteorological inquiries, and indicate the course to pursue in reference to other stations.

LX. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 394.]

December 4, 1872.—Prof. P. Martin Duncan, F.R.S., V.P., in the Chair.

THE following communications were read :—
1. " On the Tremadoc Rocks in the neighbourhood of St. David's, South Wales." By Henry Hicks, Esq., F.G.S.

The author stated that Tremadoc rocks occur in three distinct places near St. David's, namely in Ramsey Island, at the north end

of Whitesand Bay, and over a considerable tract of country about 5 miles east of St. David's. They rest conformably on the Lingula-flags, and are about 1000 feet thick in Ramsey Island. The author noticed the fossils found in these deposits, nearly all of which are of new species, and stated that the palæontological evidence proves these rocks to be nearly allied to, if not identical with, the lower part of the Tremadoc rocks of North Wales. The Upper Tremadoc rocks of North Wales seem to be represented at St. David's by the so-called Arenig rocks which overlie the deposits described in the present paper. The new species described by the author are as follows :—

> *Neseuretus* (g. n. Trilob.) *ramseyensis, quadratus, recurvatus,*
>   and *elongatus.*
> *Niobe menapiæ* and *solvensis.*
> *Theca Davidii.*
> *Bellerophon ramseyensis* and *solvensis.*
> *Palasterina ramseyensis.*
> *Dendrocrinus cambrensis.*
> *Ctenodonta menapiensis* and *cambrensis.*
> *Palæarca Hopkinsoni* and *oboloidea.*
> *Glyptarca* (g. n.) *primæva* and *Lobleyi.*
> *Davidia* (g. n.) *ornata* and *plana.*
> *Modiolopsis ramseyensis, Homfrayi, solvensis,* and *cambrensis.*

He also noticed the occurrence in the deposit of *Lingulella Davisii,* M'Coy, *Lingula petalon,* Hicks, *Obolella plicata,* Hicks, *Orthis Carausii,* Salt., and *menapiæ,* Hicks, and *Eophyton explanatus,* Hicks.

2. " On the Phosphatic Nodules of the Cretaceous Rock of Cambridgeshire." By the Rev. O. Fisher, M.A., F.G.S.

The author stated that this paper was founded upon one read by him before the Society in May last, but subsequently withdrawn, in consequence of his obtaining information which necessitated a change of opinion upon certain points. The new portion related chiefly to those nodules which had been regarded as belonging to *Porospongia* or *Scyphia,* the fenestrated structure shown in sections of which the author now identified with the structure of *Ventriculites,* as described by Mr. Toulmin Smith, the whole arrangement, and especially the presence of an octahedral figure at the nodes where the fibres of the framework intersect one another, being in favour of this determination. The author described the peculiarities of these octahedra, and dwelt particularly upon the fact that these sections of phosphatic nodules showed clearly that the fibres are really tubular, and not, as Toulmin Smith supposed, solid.

3. " On the Ventriculitidæ of the Cambridge Upper Greensand." By W. Johnson Sollas, Esq., Associate of the Royal School of Mines, London.

A collection of supposed sponges found in the Cambridge Upper

Greensand had been in part referred to the genera *Scyphia* and *Porospongia,* and in part left unidentified. An examination of sections of these forms by the microscope had revealed all the details of Ventriculite structure ; and a careful comparison with Mr. Toulmin Smith's descriptions and specimens had resulted in the identification of those examined with some four of Mr. Smith's species ; thus *Scyphia tessellata* was shown to be equivalent to *Ventriculites tessellatus* (or, more correctly, *V. texturatus*), *Porospongia ocellata* to *V. cavatus,* and other unnamed forms to *V. quincuncialis* and *V. mammillaris* respectively. The occurrence of ventriculite-structure in coprolitic material presents a favourable opportunity for a fresh inquiry into its nature ; accordingly the author described the minute characters of the hexaradiate elements of which the skeleton is composed, and the combinations of these hexaradiates with one another. Abnormalities occur sometimes by the hexaradiates becoming heptaradiate or pentaradiate, and sometimes by some of their rays bending quite away from their normal course. The whole of the skeleton fibre is distinctly tubular. Since the Ventriculite fibres have now been found fossilized in chalk, flint, and calcic phosphate, there can be little doubt that they were keratose, and not siliceous in their nature. If this be so, we have a difference between Vitrea and Ventriculitidæ of ordinal value at least, and we must look for allies to the Ventriculites among the horny sponges. *Verongia* resembles Ventriculites in the simple hollow cavity of its fibre and the non-spiculate character of its skeleton ; *Darwinella* offers a resemblance in its hexaradiate horny spicules, and *Spongionella* in the regular arrangement of its fibres. These three genera are indices of the directions in which the Keratosa tended to vary. At a very early period great variation occurred among the Keratosa, which already, at the time of the Weisse Jura, had evolved such highly symmetrical specialized forms as the Ventriculites. These, with their contemporary variations, such as Verongioid forms, lived on in great numbers throughout the Mesozoic period, with the close of which the Ventriculites altogether disappeared ; while their nearest allies dwindled down to the dwarfed and rare genera *Verongia, Darwinella,* and perhaps *Spongionella.*

LXI. *Intelligence and Miscellaneous Articles.*

AMERICAN ASTRONOMY.

APPENDIX IV. of the 'Washington Observations' for 1871 contains a most interesting account of the Founding and Progress of the United-States Naval Observatory by Professor Nourse, in which the earliest steps taken towards the establishment of an observatory, the difficulties encountered during a period of forty years in carrying out the design, the meteorological work under the superintendence of the late Commander Maury from 1844 to 1861, the devotion of the capabilities of the establishment to purely astrono-

mical work during the ten years 1861–1871, and its present equipment and *personnel* are fully set forth. The original germ of the observatory first came into existence in connexion with an occultation of η Tauri observed near the President's house on October 20, 1804, from which Mr. Lambert determined the approximate longitude of the capitol to be 76° 53′ 6‴·920 west from Greenwich. It was between five and six years after the occurrence of this occultation that a memorial was presented to the House of Representatives urging the establishment of a *first meridian* at Washington. From the presentation of a report on this memorial to the final establishment of the observatory and its entering on its work in 1844, the reader is presented with a variety of interesting documents relative to the negotiations for this great national object, which may be read with great advantage by all who cultivate astronomy. The following extracts selected from amongst these documents will convey to the reader an idea of the appreciation of the high value of astronomy by the American mind.

"The express object of an observatory is the increase of knowledge by *new discovery*. The physical relations between the firmament of heaven and the globe allotted by the Creator of all to be the abode of man are discoverable only by the organ of the eye. Many of these relations are indispensable to the existence of human life, and perhaps of the earth itself. Who can conceive the idea of a world without a sun, but must connect with it the extinction of light and heat, of all animal life, of all vegetation and production, leaving the lifeless clod of matter to return to the primitive state of chaos, or to be consumed by elemental fire? The influence of the moon, of the planets—our next-door neighbours of the solar system—of the fixed stars scattered over the blue expanse in multitudes exceeding the power of human computation, and at distances of which imagination herself can form no distinct conception; the influence of all these upon the globe which we inhabit and upon the condition of man, its dying and deathless inhabitant, is great and mysterious, and, in search for final causes, to a great degree inscrutable to his finite and limited faculties. The extent to which they are discoverable is, and must remain unknown; but to the vigilance of a sleepless eye, to the toil of a tireless hand, and to the meditations of a thinking, combining, and analyzing mind secrets are successively revealed, not only of the deepest import to the welfare of man in his earthly career, but which seem to lift him from the earth to the threshold of his eternal abode, to lead him blindfold up to the Council Chamber of Omnipotence, and there stripping the bandage from his eyes, bid him look undazzled at the throne of God."

"It is to the successive *discoveries* of persevering astronomical observation through a period of fifty centuries that we are indebted for a fixed and permanent standard for the measurement of time; and by the same science has man acquired, so far as he possesses it, a standard for the measurement of space. A standard for the

measurement of the dimensions and distances of the fixed stars from ourselves is yet to be found; and if ever found, it will be through the means of astronomical observation. The influence of all these discoveries upon the condition of man is, no doubt, infinitely diversified in relative importance; but all, even the minutest, contribute to the increase and diffusion of knowledge. There is no richer field of science opened to the exploration of man in search of knowledge than astronomical observation; nor is there, in the the opinion of this Committee, any duty more impressively incumbent upon all human governments than that of furnishing means and facilities and rewards to those who devote the labours of their lives to the indefatigable industry, the unceasing vigilance, and the bright intelligence indispensable to success in these pursuits."

From the time Commander Maury entered on his duties to the present, the course of the "Naval Observatory" has been one of onward progress. The meteorological work was great; but a greater astronomical work was proposed, that of assigning to every star in the heavens that could be seen with the observatory instruments its colour, position, and magnitude. This noble work was begun and carried on until 1849, when, the corps of computers proving altogether insufficient to keep pace with the observers, it was discontinued. Nevertheless much valuable work has been accomplished since, as a reference to the volumes and reports will show.

We cannot close this notice of the progress of astronomical science in the United States, at one observatory only, without referring to the contemplated extension of optical power in the erection of a refracting telescope of good definition and of 26 inches clear aperture, now in course of construction by Messrs. Alvan Clark and Sons. This instrument is to be completed within four years of the date of contract (June 30, 1871), or thereabouts. Doubtless in the hands of such zealous observers as constitute the staff of the observatory it will do good service to astronomical science, and contribute in no small degree to uphold the position now accorded to the United-States Naval Observatory by the spontaneous verdict of the scientific men of the Old World.

ON THE SUDDEN COOLING OF MELTED GLASS, AND PARTICULARLY ON "RUPERT'S DROPS." BY V. DE LUYNES.

The bursting produced in "Rupert's drops" the moment the thin end is broken off has been hitherto attributed to the state of forced dilatation of the interior. It is supposed that the external layer, suddenly solidified by cooling, while the inner portions are still hot and much expanded, compels the latter, to which it remains adherent, to retain a volume greater than that to which they would be reduced if the whole drop had been cooled slowly; hence a state of unstable equilibrium, which is only maintained by the resistance of the outer layers; so that when this re-

sistance is destroyed by breaking off the thin end or another portion of the drop, the state of equilibrium ceases and bursting takes place.

The experiments whose results I have the honour to present to the Academy seem to prove, on the contrary, that the effects in question are chiefly due to the peculiar condition of the exterior layers, and that the interior play no part, or only a secondary part, in the phenomenon.

The mechanical actions by means of which the drops are ordinarily broken, necessarily produce vibrations in the glass, the effect of which it is impossible to appreciate. That is why, in this investigation, I have preferred to make use of fluorhydric acid, the action of which can be moderated at pleasure, and which permits us to destroy at will, and without any shock, any portion we wish to attack.

On suspending a Rupert's drop by a thread over a platinum vessel containing fluorhydric acid, in such a manner that the extremity of the thin end dips into the liquid, we find that we can always dissolve the whole of the thin end without destroying the drop; but when the acid touches the origin of the neck (that is, the point of divergence of the pear), equilibrium is always broken; the drop then separates into a great number of fragments, and in most instances without explosion.

Reciprocally, the swollen part may be immersed in the acid, the origin of the neck and the whole of the thin end being kept out of the liquid; in this case the drop is completely dissolved without rupture, and the thin end remains intact. If with different drops the experiment is arrested at different stages of the dissolution, it is found that the nucleus which remains presents no longer the properties of the original drop; it no longer breaks up when the thin end is broken off—which shows clearly that the interior mass of the glass does not intervene in the phenomenon.

These two experiments prove at once that the stability of the drop is bound up with the existence of the origin of its neck, since whenever it is preserved no disaggregation of the drop takes place.

Now it is known that chilled glass remains more expanded than if it had been cooled slowly; the exterior layers of the drop, more strongly chilled, are more expanded than the interior layers, which have occupied more time in cooling. We may therefore regard the drop as formed by the superposition of layers of glass unequally chilled and expanded, cemented to one another. The exterior layers, kept by the resistance of the interior ones, can only yield to the force of elasticity which solicits them if, through any cause whatever, they are all at the same time set free to return to their normal state of expansion.

It results, moreover, from the form of the drop, that all these layers, unequally stretched, meet together at the origin of the neck; so that on destroying this the common point of resistance vanishes, and these layers, the actions of elasticity of which are added to-

gether, are displaced along the same directions and produce the disaggregation of the system.

If this supposition is correct, one might make a drop burst by cutting it at the large end, in such a manner as to set free at one of their extremities the unequally chilled layers of glass. This is, in fact, what takes place when the large end of the drop is ground off in a lathe, or when we attempt to saw it ; the explosion always takes place as soon as a little more than half of the thickness has been attacked.

Another consequence of this hypothesis is, that the vitreous molecules will be displaced in a direction different according to the manner in which rupture takes place. This displacement should be greater for the exterior than for the interior layers, which are less expanded ; and it is almost *nil* for the central portions, which have not been chilled at all or but very little. Consequently, on considering the molecules in a plane transverse section of the drop, the eccentric molecules, belonging to the more expanded layers, will be more displaced than those situated nearer to the centre, which will be less and less so as they are further from the surface ; so that after the rupture this plane section will have the appearance of a conic surface formed of little needles of glass, arising from the shrinkage on all sides ; and the direction of the summits of these sorts of cones will indicate that in which the displacement of the molecules has taken place.

If the thin end be broken off, the shrinking will be towards the bulb, and all the summits of the cones will be directed towards the thin end. If the explosion be caused by sawing the big end of the drop, the cones will have their summits directed towards the big end.

That this is what in fact happens I have ascertained by operating in the following manner. I fit some Rupert's drops in plaster, covering only a little more than half of their thickness. The thin end, which is left protruding, I immerse in fluorhydric acid. The moment the neck is attacked the drop is disaggregated, with or without explosion ; and the fragments constitute, by their grouping, a series of conic assemblages encased one within another and having their summits towards the thin end. On sawing the big end, the summits have the opposite direction ; and if the drop is sawn in the middle, the two opposite arrangements are observed on the two sides of the incision. Operating while the plaster is fresh, we can easily detach the fragments of the drop and establish all the results I have stated.

These facts demonstrate that in the drop the glass is in no peculiar condition other than that which arises from the unequal expansion resulting from the difference of cooling.

Analogous phenomena are presented by thick glass rods which are chilled naturally by cooling in the air at the moment of their fabrication. When these rods (which have always some curvature) are heated at one end, it sometimes happens that they break along

their whole length, the fracture being conic and acicular. I am
indebted to the kindness of M. Friedel for a fine specimen of this
kind. The tubes obtained by letting melted glass flow in threads
of more or less thickness into water possess in a high degree the
explosive properties of Rupert's drops. They have almost always
the form of cork-screws, on account of the extreme expansion of
the upper layers; and dipping the extremity in fluorhydric acid is
sometimes sufficient to cause an instantaneous explosion, with the
same characters in the fracture. In fine, the lumps of glass which
remain at the extremities of the canes by means of which the tubes
are drawn have the form of large Rupert's drops, and are of consider-
able weight. When these are detached from the cane they are in
the condition of a drop of which the bulb has been sawn : during
their cooling in the air they break up, throwing off splinters with
violence ; and their fracture is identical with that of the small drops
broken at the big end. A fragment of one of these large drops,
which I had brought from the works of MM. Appert at La Villette,
presented an interesting phenomenon : on slightly squeezing it
between my finger and thumb, a considerable disengagement of heat
was produced ; the temperature rose to about 40° C. This confirms
the results obtained by M. Dufour concerning the heat disengaged
during the explosion of Rupert's drops.

The existence of layers cooled with unequal suddenness in the
thickness of the glass affords an explanation of the brittleness of
chilled glass. In fact we may suppose that, on account of the low
heat-conductivity of glass, a very thin layer at the surface is, from
whatever cause, cooled with sufficient suddenness to be in a very
different state of expansion from that of the layers beneath. The
least shaking, or the slightest change of temperature, will cause it
to break ; and the fissure will be propagated in the mass of the
glass : exactly the same thing takes place when a crack shows itself
on pottery the glaze of which has been ill-compounded.—*Comptes
Rendus de l'Académie des Sciences*, vol. lxxvi. pp. 346–349.

ON A METHOD OF MEASURING INDUCED CURRENTS.
BY F. H. BIGELOW.

If a Wheatstone's bridge be formed, in which the secondary coil
of the inductorium is the resistance R_1 to be measured, the relation
between the resistances will be expressed by the proportion $R_1 : R_2$
$= R_3 : R_4$, where R_3 and R_4 are in a fixed ratio. By passing a cur-
rent from an independent battery through the primary coil, the
strength of the current in R_2 will be increased or diminished on the
breaking or making of the inducing current. Therefore, to preserve
equilibrium in the bridge, R_1 must be changed, and we shall have
$R_1 \pm C : R_2 \pm x = R_3 : R_4$, in which x is a resistance equivalent to
the effect of the induced current. Let $R_1 \pm C$ be found by trial
such that the addition or subtraction of the induced current will

produce equilibrium in the bridge—that is, will be such as to bring the deflection of the galvanometer to the zero of the scale. We shall then have the strength of the induced currents expressed as resistances. One advantage of this method is this—that the readings are always reduced to the same point, the zero of the scale. This method also has a wider range than that of merely taking the swing of the galvanometer-needle; for currents which would throw the spot of light in a reflecting galvanometer off the scale can be readily kept on the scale in this method by merely altering the ratio of R_3 to R_4 in the Wheatstone's bridge. Since the shunting of induced currents is accompanied with difficulties, this method is especially advantageous. When the bridge was set up so that the smallest variation in the resistance of the branch containing the inductorium gave the greatest variation in the current going through the galvanometer, namely when $\dfrac{dS_0}{dR_3} = 0$, S_0 being the current through the galvanometer, and the resulting value of R_3 being

$$R_3 = \sqrt{\frac{GR_1(2B + R_1)}{G + 2R_1}},$$ in which G is the resistance of the galvanometer, B that of the circuit exterior to the Wheatstone's bridge, it was found that the induced currents could be measured to one hundred-thousandth of an ohm.

The following Table contains a comparison of the induced currents produced by making and breaking the circuit. The first two columns contain the variation in ohms of the variable resistance of the bridge; the third and fourth columns give the strength of the induced currents on making and breaking, expressed in ohms.

Change in the resistance on breaking.	Change in the resistance on making.	Strength of Induced Current on breaking.	Strength of Induced Current on making.
650	600	·00325	·00300
700	680	·00350	·00340
720	720	·00360	·00360
720	750	·00360	·00375
700	700	·00350	·00350
850	850	·00425	·00425

Care should be taken to send the induced currents to be compared in the same direction by means of a pole-changer. It will be seen from the above Table that the equality of the currents on making and breaking can readily be proved by this method.—Silliman's *American Journal*, May 1873.

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END OF THE FORTY-FIFTH VOLUME.

ALERE FLAMMAM.